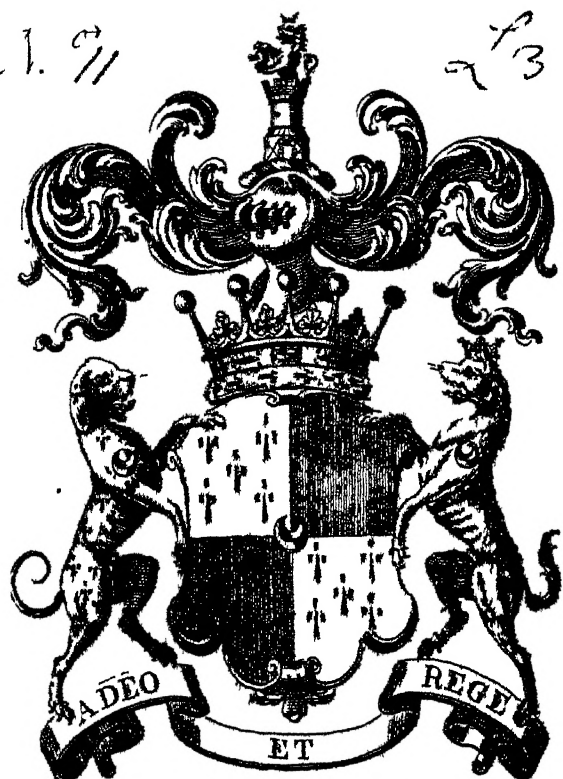


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Philip Earl Stanhope.

PHILOSOPHICAL
TRANSACTIONS,
OF
THE ROYAL SOCIETY
OF
L O N D O N.

V O L. LXVI. For the Year 1776.

P A R T I.

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A D V E R T I S E M E N T.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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PHILOSOPHICAL
TRANSACTIONS.

- I. *On the Nature of the Gorgonia; that it is a real Marine Animal, and not of a mixed Nature, between Animal and Vegetable. By John Ellis, Esq. F. R. S. in a Letter to Daniel Solander, M. D. F. R. S.*

MY DEAR FRIEND,

R. June 29, 1775. **I**T was your particular request, before you went to the South Seas, that I should continue my researches into the formation and growth of *Zoophytes*, more particularly of those formerly called *Ceratophyttons*, now *Gorgoniæ*; and known in English by the name of sea-fans, sea-feathers, and sea-whips, to which class the red coral should be added. This you thought the more necessary, as the accounts already published of them by the illustrious Dr. LINNÆUS and Dr. PALLAS seemed to make them of a mixed nature in their growth, between animals and vegetables: a thing not easily to be reconciled to the usual operations of nature.

I was so fortunate about that time to receive from my right honourable friend the earl of HILLSBOROUGH, a most excellent collection of different species of these animals preserved at the sea-side in spirits, by JOHN GREG, esq. F. R. S. of Dominica. This hath enabled me to shew more clearly, that they are true animals, growing up in a branched form, and in no part vegetable.

From the following observations it will appear, that the *gorgonia* is an animal of the *polype* kind, resembling the common fresh water *polype* in many of its qualities, but differing from it in the remarkable circumstance, of producing from its own substance a hard and solid support, serving many of the purposes of the bone in other animals. Every one knows, that the common *polype* sends out its young from its side, like buds, which being grown to the form of the parent animal, to which they still adhere, send out again their own young, like buds, adhering to themselves; and this is repeated, till at length the whole acquires a branched appearance, resembling a vegetable, see fig. 1.

The *gorgonia* grows nearly in the same manner; and hence arises its resemblance to a shrub, which hath given occasion to the mistake of placing it in the vegetable kingdom. But though the nature of these animals is so much like the *polypes*, they differ in several circumstances; the most remarkable is that which I have already mentioned, the hard bone by which the *gorgonia* is supported. This is not formed by any kind of vegetation, but by a concreting juice thrown out from a peculiar set of longitudinal

tudinal parallel tubes, running along the internal surface of the fleshy part. In the coats of these tubes are a number of small orifices, through which the *offeous* liquor (if I may use the expression) exudes; and concreting, forms the layers of that hard part of the annular circles, which some, judging from the consistence rather than the texture, have erroneously denominated wood.

Dr. PALLAS, in his *Elench. Zoophytorum*, p. 162. is of opinion, that the layers of which the wood, as he calls it, of the tougher *gorgoniæ* is composed, may be separated into numerous longitudinal fibres; that the longitudinal *striae*, which frequently appear on its external surface, are owing to this structure; and that these fibres are in fact hollow, like the wood of trees, the cavity of the tubes being closed up, as they become hard and rigid.

I was nearly of the same opinion when I was writing my *Essay on Corallines*, as may be seen in the Philosophical Transactions. vol. XLVIII. p. 18. and also p. 504. t. 17. where I have compared the herring-bone coralline, which is composed of many little tubes, to the growth of sea-fans and sea-feathers, now called *gorgoniæ*; and likewise in my *Observations on the Growth of the red and white Coral*, see Philosophical Transactions, vol. XLVIII. p. 504. t. 17.; but experience has since fully convinced me of the contrary: for upon the strictest examination with the microscope, of the internal horny parts of several of those *gorgoniæ* fresh from the sea, and immediately preserved in spirits, not the least appearance of tubes within the horny part can be discerned, either in the longitudinal or transverse sections. There

is indeed a regular cannulated appearance on the surface; but this seems to be only an external moulding, and not formed by a series of longitudinal tubes with interstices, as in plants; nor is it difficult to explain from whence such a moulding may arise. I have observed, that the inner surface of the fleshy part contiguous to the bony or horny part, is furnished with longitudinal parallel tubes, which through certain pores supply the *osseous* matter; this being soft at first, and only afterwards becoming hard, so as necessarily to take the form of the concave surface by which it is closely pressed, and therefore assumes a striated appearance. This is plainly seen in fig. 2. A. where the ends of the tubes and the striated appearance on the *gorgonia flabellum* are expressed; and at fig. 2. B. two of them are magnified.

In the *Isis bippuris*, or black and white jointed coral, which is very nearly a-kin to this *genus*, these tubes are still more clearly to be seen, as they are larger and the channels much deeper, see fig. 3. where A is a part of the coral of its natural size; B is an extremity of one of the branches magnified, with the bony part laid bare; c a part of the same, with the bony part taken out, to shew the tubes with their internal orifices, through which the *osseous* juice is supposed to exude, and form the layers of the bony and horny part. This formation of the hard part or bone of the stem seems to be a principal use of the longitudinal tubes; but they have another also, of great consequence in the growth of the *gorgonia*: for it is by means of these, that the animal spreads itself downwards over the substances which serve for its basis, thence
deriving

deriving a firmness proportioned to its bulk. By means of these likewise, it repairs any deficiencies arising either from accident or natural decay, by which the life of the whole would be endangered. At fig. 2. c, d, the broken stem in the *gorgonia flabellum* is strengthened and made firm by the lateral reticulations being covered over with the horny substance by means of these fleshy tubes and *polype* suckers. This is very different from any natural repairs of broken or wounded branches in trees. Besides, these tubes extend themselves any way, creeping over every substance which may serve for their support and preservation of the animal, throwing out the horny or *osseous* juice to make the whole texture firmer. This wonderful contrivance of nature is certainly instinct in this low order of animals. To give a better idea of this kind of instinct, and to shew in what it differs from what is called radication in plants, with which some people, for want of better information, are apt to confound it, I have given a figure of the manner in which the *flustra foliacea* fastens itself to shells, see fig. 4. This figure is a little magnified, to shew the form of the cells, as they have spread themselves over the surface of the scollop shell. The advocates for the vegetation of *zoophytes*, I hope, will be convinced, that the part that sticks to the shell is not a root, but only a single course or layer of cells of the same animal. As it rises into leaf-like branches they become double, or two layers of cells, placed in such an opposition to one another as to strengthen the whole, like the cells in the honey-comb; and what is very singular, the narrow part of the stem near the shell, often

consists

consists of four or more layers of cells, which the animal, by this kind of instinct, most certainly applies to strengthen that slender part against the force of the waves. For another instance of the base of a *zoophyte* spreading downwards to secure itself, we have an example in the *madrepora muricata*, which is extending itself over a dead animal of the same species, see fig. 5.

The following remark of Dr. PALLAS will shew, that as he conceives the wood or horny stem to be composed of tubes, so he thinks that there is a communication of juices from the *polypiferous* pores on the cortical part to the inside or horny part, as in trees: for he observes, that as the trunk of the *gorgonia* is always proportioned to the size of its branches, the wood or horny part of the trunk, notwithstanding its hardness, must necessarily thrive, grow and increase every way, even though the organs of the bark, or surrounding fleshy substance, at the trunk and base are obliterated^(a); and hence he concludes, that the trunk must receive nourishment from the branches, and apprehends, this nourishment to be absorbed and prepared by *polypiferous* pores. Now it is evident, that the idea of the trunk and base of a tree growing in thickness, when it is divested of its surrounding bark, is contrary to the known laws of vegetation. The only method of increase in the trunks of trees is by

(a) Semper baseos amplitudo et imi trunci crassities proportionata magnitudini fruticis reperitur; argumento corneam eorum partem, quam exemplo arborum fruticumque, lignum dicere queat, obstante duritie ubique vigere, vivere et in omnem dimensionem crescere, obsoletis quamvis corticis in trunco et basi organis. PALLAS, Elench. Zoophyt. p. 161.

the apposition of new layers from the bark, which cannot be produced but while the bark is subsisting.

Nor can the *gorgonia* increase in size, in those parts where it is deprived either of the flesh with the *polype* suckers, or the surrounding fleshy tubes, which communicate with these suckers; for these suckers and tubes are the organs that prepare and deposit the several thin layers, which form the support or bony part (here called wood), as I have shewn before. If upon examining the internal structure of these *zoophytes* it were found, that their growth and fabric any ways resembled that of vegetables, this would indeed afford a presumptive argument, that they did participate of a vegetable nature. Yet even in that case, it would be much more reasonable to suppose them animals of the lowest order, raised but one degree above the vegetable tribe, than to conjecture a monstrous metamorphosis repugnant to the general analogy of nature. But the truth is, that although the hard parts of many *gorgoniæ* have very much the external appearance of wood, yet the internal structure differs in the most essential points from vegetables.

In order to prove this, I have compared different sections of the *gorgonia* with correspondent sections both of sea and land plants, and find they differ in the following particulars: The longitudinal sections of the stems of the larger *fuci*, such as the *fucus digitatus*, *esculentus*, *nodosus*, and *saccharinus*, appear composed of parallel jointed tube-like figures, the joints of which are composed of gland-like cells; these tubular appearances, when highly magnified, are discovered to be connected together

together by transparent reticulated fibres, or very minute transverse tubes, interwoven with the upright ones. In a horizontal section, the ranges of cells, which look like rays from the center, as they approach the bark, grow smaller and smaller, and most probably correspond with the minute pores which cover the outward surface of the plant; for when the sides of the dry stems are soaked in water, they quickly imbibe it, and soon become full of a gelatinous liquor; all which is totally different from the texture of the *gorgonia*.

We come now to compare them with land plants, such as shrubs, to which they are generally supposed to grow like. The *gorgonia* has no regular series of hollow fibres or little tubes, in what is called the wood, either longitudinal or horizontal. It appears composed of a sort of irregular *laminae* like horn; the fibres of which take no certain direction, nor preserve in any two places the same thickness. It has no series of utricular vessels, as the transverse vessels of wood are called by MALPIGHI; or insertions as they are called by Dr. GREW. These are essentially necessary, as forming a communication from the bark and the internal parts of the wood quite through. On the contrary, the concentric circles of the *gorgonia* have no connexion with each other; they run like so many parallel curves, and are connected by no insertions or utricular vessels; but to all have been appearance formed by separate depositions of concreting matter. So the shells of snails and oysters are formed; their respective animals throw out periodically the *osseous* juice or *testaceous* matter, which adheres to the former shell and concretes, and thus

Fig. 2.

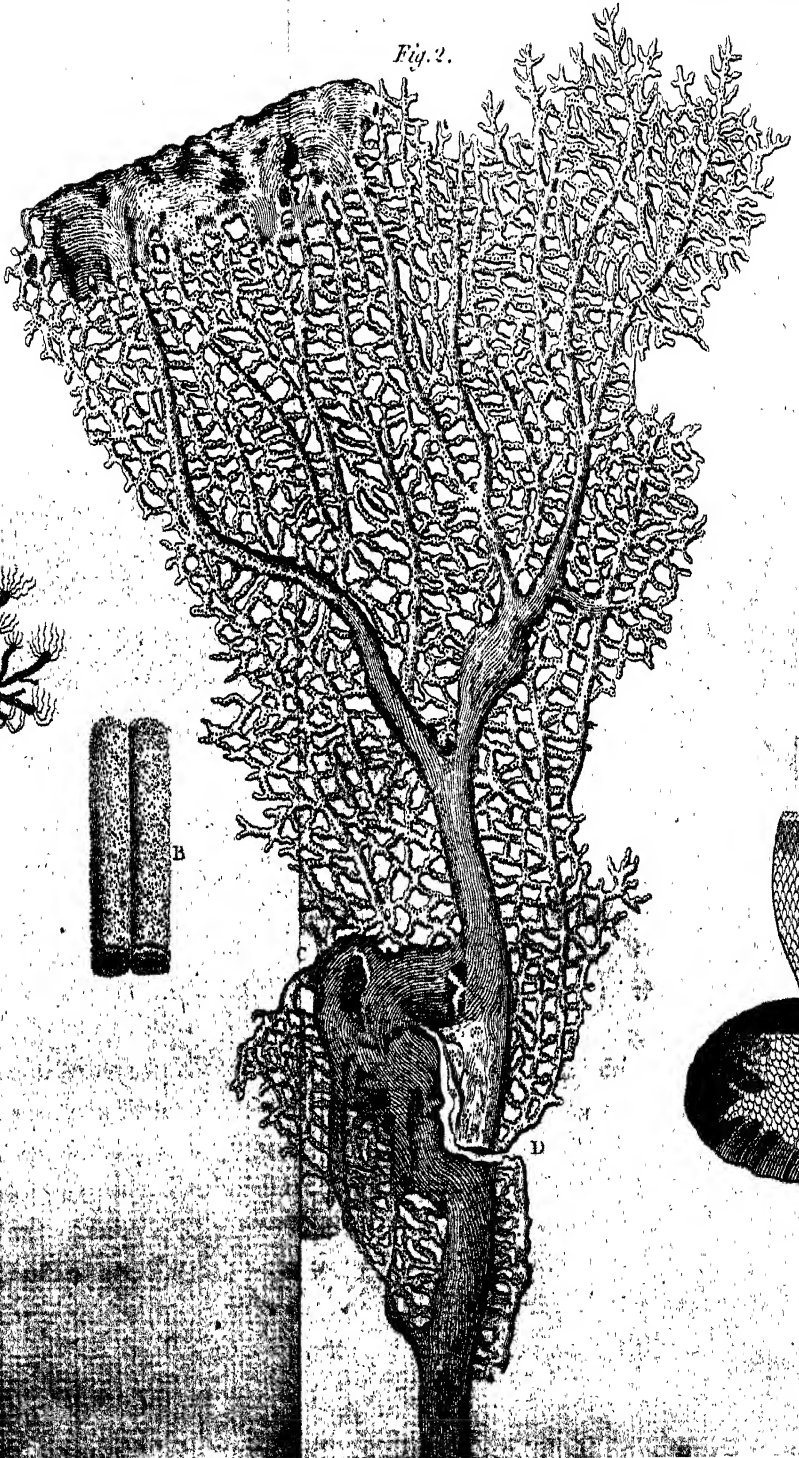


Fig. 1.

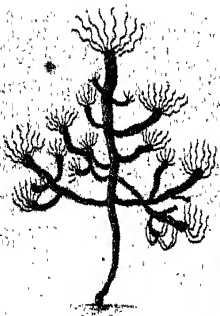
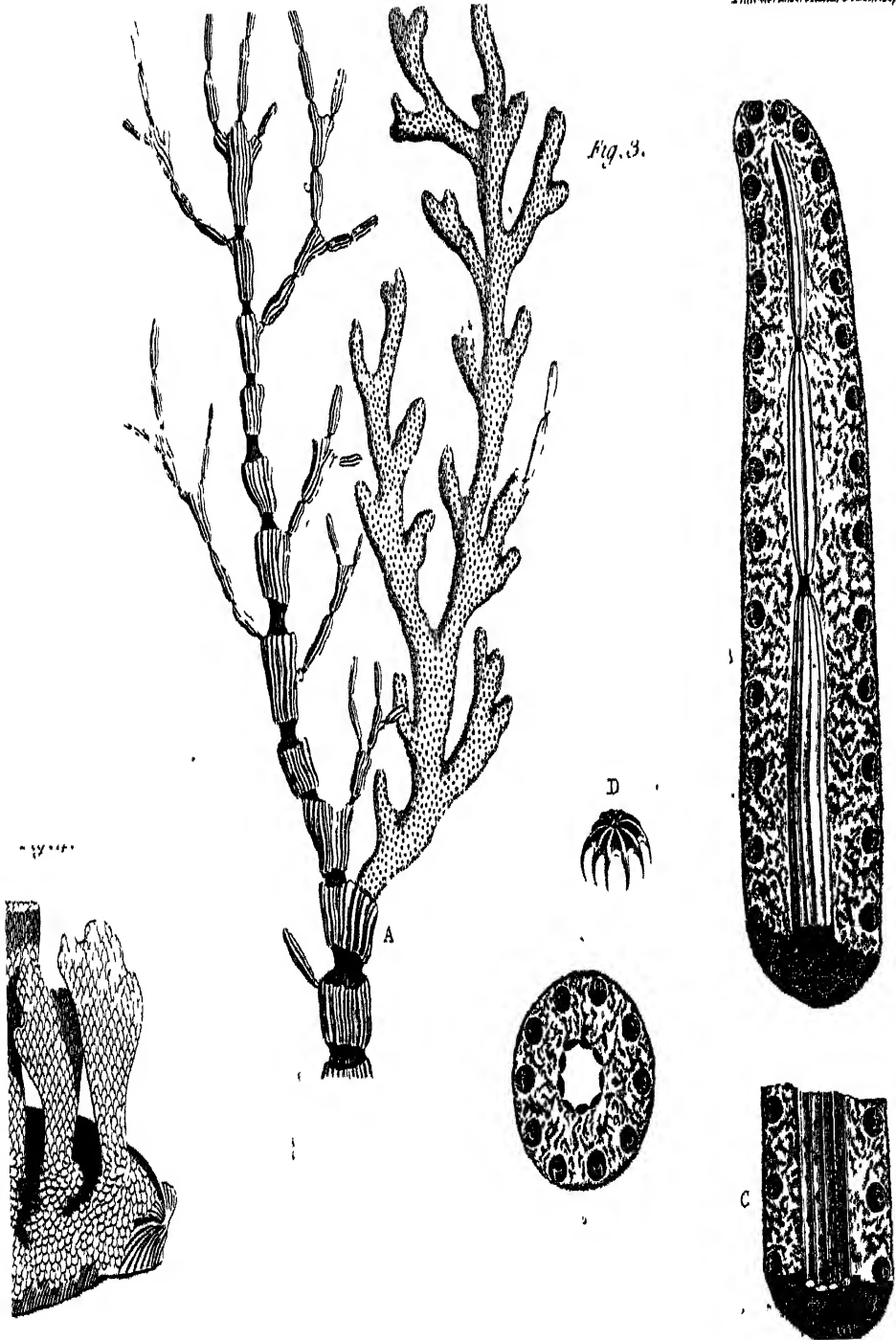


Fig. 3.



thus successive layers are produced. In the same manner I suppose the concentric circles of the *gorgonia* to be formed, successive layers of juice exuding from the fleshy tubes that surround the hard part or bone of the animal. Thus the stem of the *gorgonia verticillata*, or *Minorca white sea-feather*, is composed of different layers of a shell-like substance (see fig. 6.), where a broken part of the stem is represented, and a piece of it magnified, to shew that there is evidently no more communication between the different *laminae* than there is between those of an oyster-shell. In a transverse section of the *gorgonia pretiosa*, or true red coral, DONATI has observed, Philosophical Transactions, vol. XLVII. p. 97. t. 3. f. d. " Different lines or annual bands, whereof one part is of " a rose colour, another yellowish, others white, others " more or less charged with colours, that form concentric circles like the coats of an onion." This diversity of colours could hardly have taken place, had there been a circulation of juices through the stem; and it was probably owing to the different food which the animals had lived upon at different periods.

There is another *genus* of *zoophyte*, which though it swims freely about in the sea, yet approaches near to the *gorgonia*, and will serve further to explain the growth of its stem, and that is the *pennatula*, or sea-pen. This *genus* hath a bone along the middle of the inside, which is its chief support. This bone receives the supply of its *osseous* matter by the same *polype* mouths, that furnish it

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with

with nourishment. Dr. BOHADSCH has very judiciously brought to this *genus* the great Greenland clustered *polype* formerly described by me under that name, and now called *pennatula arctica*. In a cross section of the bone, see Philosophical Transactions, vol. XLVIII. tab. XII. F. H. the several *lamine* are magnified, to shew that they are formed in layers like shells, and are not full of tubes as in a vegetable growth. These animals are ranged among the vegetating kind, and so called by Dr. PALLAS. There is a great affinity between the *gorgonia* and *isis*, so that the increase of the bone of the latter will greatly illustrate that of the former. The longitudinal section of the bone to the stem of the *isis bippuris* will shew, that it hath been increased in diameter by successive layers of stony matter that surround it, see fig. 7. In this instance we can trace the bone in its infant state, when nature had given it pliable black horny joints, that it might yield the better to the violence of the waves; but as soon as it became stronger, these horny black joints were no longer necessary, as we find the lower part of the stems totally overgrown with the bony substance. The furrows in this coral are deeper than those of any other; insomuch that not only the longitudinal fleshy tubes that surround the bone, but even the minute pores in them, through which the *osseous* juice exudes, are very discernible see fig. 3.

We now come to a very singular circumstance in the growth of the *gorgonia*, in which it differs remarkably from that of trees. In fig. 8. is the figure of the naked
stem.

stem or bone of a *gorgonia*, to which we find several tree oysters and other shells have adhered. These shell-fish seem to have killed the *gorgonia*; for the same stem seems to be covered over with another *gorgonia* of the same kind; which in its growth hath almost covered the shells, and likewise the branches to which they were fastened, leaving only part of the ends of the branches of the first *gorgonia* yet uncovered. The size and weight of the shells probably gave the waves so great a power over the stem, that it was at last broken off, and cast on shore in the state in which it is here represented. This instance of a *gorgonia* growing over one of its own kind, seems sufficient to account for the circle of calcareous matter found now and then in the cross sections of old stems, between the horny circles, as hath been observed by Dr. PALLAS, Elench. Zooph. p. 162. "Interjecto quandoque tenui materiæ calcareæ strato." But, I believe, no one hath ever seen the bark of trees inclosed in the same manner in the inner circles of the wood; and indeed it is so contrary to the laws of vegetation, that Dr. PALLAS hath not attempted to account for it, by shewing any parallel instances in the transverse sections of timber. Another instance of the manner of growing of these animals is still more remarkable, see fig. 2. where the upper part of the *gorgonia flabellum* meeting with an obstruction in growing upwards, has grown downwards over its own fleshy substance, and has evidently inclosed and covered over its own reticulated branches, with a continuation of its own flesh and bone. Dr. PALLAS, in a note on the growth of the *gorgonia*, hath the following extraordinary

dinary observation, that a gentleman in Holland is possessed of a *gorgonia*, which has on the same shrub, the bark partly of a *gorgonia verrucosa*, and partly of the *gorgonia coralloides*, without any visible difference of the branches; which he accounts for by comparing it to the growth of vegetables, saying: “ So different *lichens* are
 “ often found incorporated in such a manner together,
 “ that they might easily be mistaken for one and the same
 “ plant^(b). But I think it rather paradoxical to suppose the flesh of one animal to grow on the bones of another. If he had examined it attentively, he would have found what we have advanced to be the case. It is not unusual for a *gorgonia* of one species to grow upon the decayed branches of an individual of another, where the soft or fleshy part is already perished; but the upper or living *gorgonia* must have its own hard as well as soft parts; for should there be the fleshy part, and not the bony part, it would belong to the *genus* of *alcyonium*, and occasion such another remarkable mistake as this author has already made in his *fertularia gorgonia*, see Elench. Zooph. p. 188. where he has described an *alcyonium*, growing upon and surrounding the stem and part of the branches of the *fertularia frutescens*, as a new species of *fertularia*. This, he says, most closely unites the *genus* of *gorgonia* with that of the *fertularia*: and to convince me of the

(b) Quod in eodem frutice corticem partim gorgoniæ verrucosæ partim coralloides exhibet, sine ullo visibili discrimine ramorum. Verum et diversæ sæpe lichenes ita sibi invicem inolitæ reperiuntur, ut pro unâ facile plantâ sumeres.
 PALLAS, Elench. Zooph. p. 163.

truth of what he asserts, he has sent me part of the original specimen, of which fig. 9. exhibits an exact representation. At A is a magnified figure of this *alcyonium*, on a piece of the branch of the *fertularia*. It is of a fleshy substance with warts, having each twelve rays; we have many species of *alcyonia* from the West Indies not much unlike this. The reader, by attending to the Doctor's own description of his *fertularia gorgonia*, will soon be convinced of the error, especially when he considers, that the character of a *fertularia* is that of a branched animal, with the hard parts without, and the fleshy parts within; and that the *gorgonia*, on the contrary, hath its fleshy or soft parts without, and its bone or hard parts within.

There is another essential difference hitherto unnoticed, between the growth of the *gorgonia* and that of trees; and that is, in the connexion between the side branches and stem of the one, and the side branches and stem of the other. The side branches of vegetables proceed from the pith; of course, when a stem and side branch is divided length ways, the pith is seen continued through the main stem into the branch, see fig. 10. where A. is the natural size of a small twig of the lime tree, and B. the same magnified. It must be observed, that in some trees the channel or continuation of the pith which leads from the stem to the side branch, is very much contracted; and the communication very narrow; in which case it will be necessary to make cross sections, which will soon discover the course of the pith from

from one to another. M. DU HAMEL, an author of the first reputation, hath clearly demonstrated this in his *Physique des Arbres*, vol. II. p. 119. tab. II. f. 91. Now in the *gorgonia*, the support, or what is called the woody part, is indeed furnished with a kind of a pith or *medulla*: but when we cut the stem or branch through the middle lengthwise, we find no passage whatsoever between the pith of the stem and that of the branch, each being surrounded with a hard covering of its own, which hath no perforation, nor admits of any communication. Every branch of a *gorgonia* therefore hath its own pith or *medulla* peculiar to itself, which is never found passing into that of another, see fig. 11. A. the natural size, B. magnified. Again, in trees, the pith is largest in young shoots, and disappears in old stems: in the *gorgonia* the *medulla* is of the same diameter in the old stems as in the young branches. In the longitudinal sections of fresh shoots of trees, the pith in the microscope looks like a number of jointed tubes united together; and in the cross sections, it appears like so many circles. In dried specimens the tubular appearance in the longitudinal sections is more irregular; they look rather like longitudinal ranges of little transparent blebs, and the cross sections appear like circles intersecting one another in the margin; but there are many varieties of figures in the pith of different vegetables; what is mentioned here, is the common appearance of pith in most plants. When we cut a dry branch and stem of a *gorgonia* through the middle lengthwise, the pith appears divided into many little transverse membranes, like small

white *diaphragms*, separated from one another about the distance of their own diameter. But these cross membranes are found to be more numerous in such as have been preserved directly from the sea in spirits; and when they are examined in the microscope, they appear to be of the nature and substance with the *lamine* that compose the horny tube that surround them^(c).

(c) While I was comparing the longitudinal sections of the young branches of trees with those of the *gorgonia*, I was surprized to find such a similitude between the pith of a branch of a walnut tree, of a year's growth, and that of the *gorgonia*, see GREW, *Anat. of Plants*, tab. xix. fig. 4. A. and B.; especially as we are told by a modern author, who hath published many microscopical observations on the construction of timber, that the cell-like divisions in the branch of a walnut tree are only a row of single blebs of pith. But the microscope discovers to us, on viewing one of these cross membranes, that it is composed of many cells shrunk up and united together; for, upon viewing the flat surface of one of them, it appeared full of circles intersecting one another, like a thick transverse section of many other dried piths pressed together: besides, the thicker part of this shrunk-up walnut pith, all round next the inside close to the wood, when magnified, plainly shewed the same appearance of blebs as in other pith. To confirm this observation, May 23, 1772, I procured a young green shoot of a walnut tree, growing from a branch of the preceding year; and examining the pith, both in upright and transverse sections of this new shoot, I found that they exactly resembled the pith of many other trees, but were full of sap: and that the ranges of cells or blebs that occupied one of these spaces could not be less than a hundred, perhaps double that number of blebs. Dr. GREW takes notice, p. 120. in his *Anatomy of Plants*, that there are other trees beside the walnut tree whose pith in the last year's shoot shrinks up and forms such cavities; and an ingenious friend of mine, now engaged in an enquiry into the structure of plants, hath shewn me a last year's stem of the *brassica sylvestris*, or shrubby cabbage, whose pith is shrunk and divided into a single row of cells, like those of the walnut tree of last year's growth.

I come now to the outside covering or skin of the animal. As few have been at the pains to examine the surface of the *gorgonia* accurately, it hath scarcely yet been noticed, that they are cloathed with a kind of scales, and some of them so remarkably covered, and the scales so well adapted to the particular parts, that one might reasonably be induced to think, that nature hath given them this defence, as she hath done in like manner to the several parts of snakes and lizards, as a kind of armour to protect them from external injuries. As instances of the above, I shall only mention, that the surface of the stem as well as the mouth of the cells of the *gorgonia placomus* are defended by long pointed scales, see *Essay on Corallines*, p. 27. t. A. I. to 3.; and the *gorgonia verticillata* (of which an elegant specimen is to be seen in the British Museum) hath also very remarkable scales of different sizes round the mouths and on the skin, see *Essay of Corallines*, t. 26. f. s. T. The *gorgonia lepadifera* hath a most remarkable variety, placed like tiles, one over another, for the defence of the mouth of the cells that inclose the *polype* suckers; besides, there is a small kind of scales, that covers the surface of the stem and branches, see fig. 12.

From the skin we are naturally led to speak of the flesh of the *gorgonia*, or what the modern naturalists call the bark or *cortex*. Whoever hath examined the flesh of the *gorgonia*, well preserved at the sea-side in spirits, will find, on dissecting them, proper muscles and tendons for extending the openings of their cells; for sending
forth

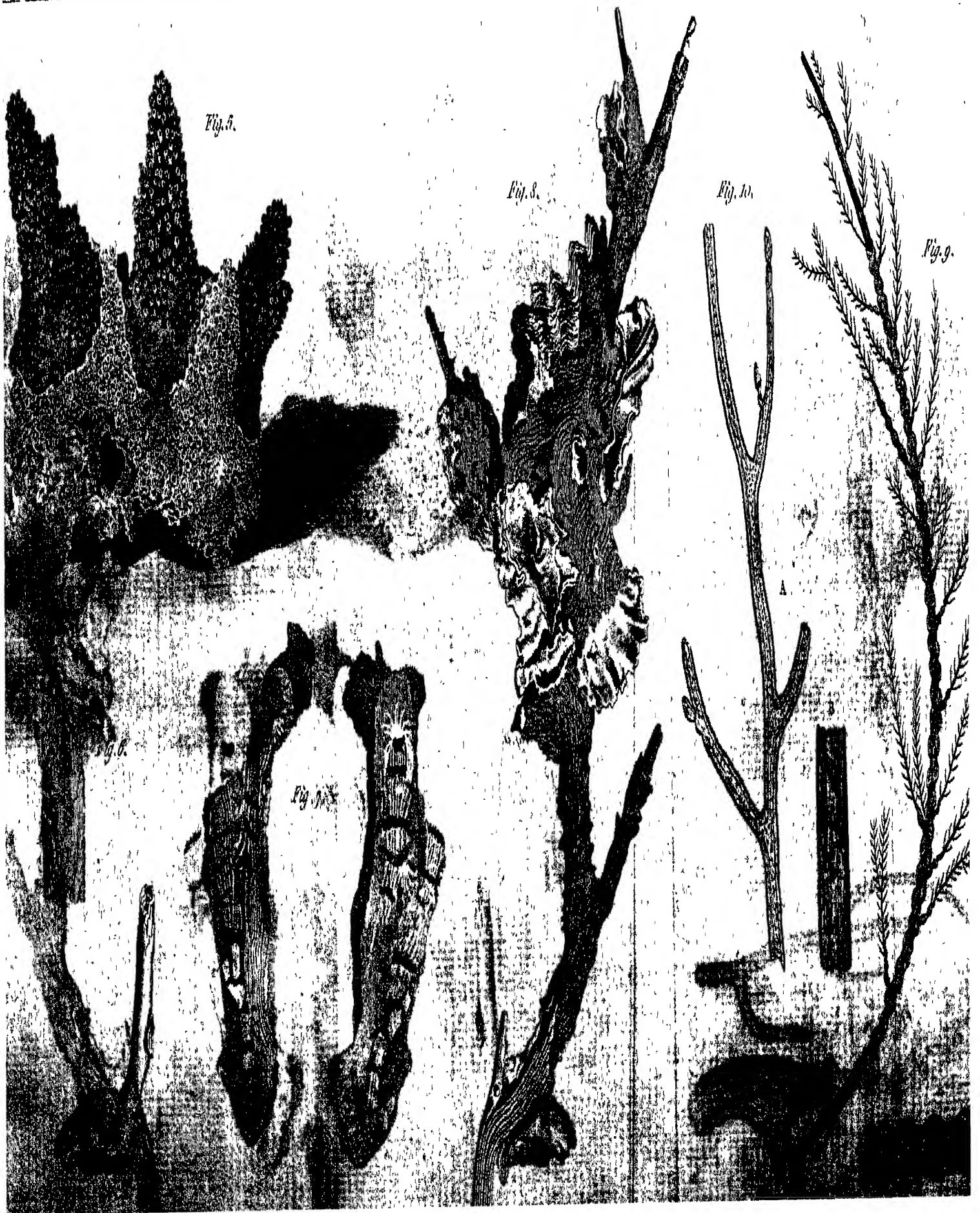


Fig. 12.



forth from thence their *polype* suckers in search of food; for drawing them in suddenly, and contracting the *sphincter* muscles of these starry cells, in order to secure these tender parts from danger; and likewise that there is, as we have already mentioned, proper secretory ducts, to furnish and deposit the *osseous* matter, for the supply of the bone, both of the stem and branches as well as the base, to secure its station with firmness, amidst the boisterous element where it is appointed to be. That there are *ovaries* in these animals is without doubt; for in most of those that were sent to me preserved in spirits, the eggs were very visible upon making longitudinal sections in the same manner and form as in the *alcyonium digitatum*, called *dead man's band*, see Philosophical Transactions, vol. LIII. tab. xx. fig. 11. but much larger; and it is very probable, many of these animals are *viviparous*, as we have seen among the *fertulariæ*.

So that I must conclude, that though they grow in a branched form, they are no more allied to vegetables than they are to the ramified configurations of *sal ammoniac*; to the elegant branched figures in the Mocha and other Gems, called *dendrites*; to the *arbor Dianæ*, or the arborescent figures of the Cornish native copper: consequently, that animal life doth not depend on bodies growing according to a certain external form. Hence it appears, that this metamorphosis of a plant to an animal is a flowery expression, and in my opinion, better suited to the poetical fancy of an OVID, than to that precise method of describing which we so much admire in a natural historian.

II. *The Variation of the Compass; containing* ^(a) *1719 Observations to, in, and from, the East Indies, Guinea, West Indies, and Mediterranean, with the Latitudes and Longitudes at the Time of Observation. The Longitude for the most Part reckoned from the Meridian of London; if otherwise, it is taken Notice of in the Margin. By Mr. Robert Douglass. Recommended to the Public by the late Dr. Halley. Communicated by the Rev. Nevil Maskelyne, Astronomer Royal, F. R. S. with a Letter prefixed from William Mountaine, Esq. F. R. S. to Mr. Maskelyne.*

TO THE REV. NEVIL MASKELYNE, A. R. F. R. S.

DEAR SIR,

St. John's, Southwark,
Nov. 24, 1774.

R. June 1, 1775. **M**R. ROBERT DOUGLASS'S observations of the variation, taken by himself, and signed by Dr. HALLEY, I now beg leave to return, with answers to the questions you proposed thereupon; viz. Whether I had ever seen them; or had made any use of them in my former publications? And if so, whether any further notice of them was necessary? In answer to the first; among my papers on the variation I have an exact copy of them, recommended under the same signature, of the same date, and in the same words, and the

(a) DOUGLASS calls these 1719 observations; but these tables, I rather think, should be called the Result of 1719 Observations. W. MOUNTAINE.

whole seems written by the same hand; so that both of them (containing 1719 observations each) may be filed originals: I have sent mine herewith for your inspection. To your second question, I answer, that I could not make any use of them in the construction of either of my charts, as the first was formed from actual observations made about the year 1744, and DOUGLASS's conclude in the year 1735; but as many of them as suited the periodic times were taken in, and compared with many thousands of others, according to the order of their respective dates, in forming the fix periodic reviews, as published in the Philosophical Transactions, vol. L. part I. for 1757. To the third question; *viz.* whether they are worth a public record? I rather think they are; and have sometimes thought to present them to the Royal Society for their approbation: for, notwithstanding they have been thus made use of, yet they are mixed and moulded up with others, and those only to every five degrees of latitude and longitude; so that the variations respecting the several latitudes and longitudes do not singly appear. And as the true theory of this *arcanum* in nature is yet so little known, every thing that serves to illustrate it, deserves attention. And as these tables, for a single set, are extensive, and may be esteemed authentic, they may very well serve for comparatives in future times; for if the change of the variation be regulated by any constant law, which I still continue to doubt, the discovery of that law must greatly depend upon such comparisons made from multitudes of good observations

taken at different periods, and those over the whole face of the terraqueous globe; but until that law is certainly known, charts can be constructed only from time to time from the latest observations. I am, &c.

W. MOUNTAINE.

P. S. From the differences, times considered, between these identical observations of DOUGLASS, and Dr. HALLEY's chart in 1700, Mr. CHARLES LEADBETTER attempted, with great labour, to delineate a set of curves for the year 1740, but without success. He seems, by his unfinished work, to have plunged himself into an inextricable labyrinth. I have the manuscript chart and his table of differences now in my custody. His attempt was frustrated by the opinion he possessed of an uniform and regular mutation; and this failure further convinced me of the necessity of proceeding upon other principles.

On the outside leaf of each copy is the following testimonial:

Mr. ROBERT DOUGLASS having shewn me this curious collection of Observations of the Magneticall Variation, made by himself in most parts of the world, I thought them worth preserving, and as such recommend them to the public, believing they may be of use both to the present and future navigation.

Greenwich,
Aug. 28, 1738.

EDM. HALLEY.

To the right honourable the Lords Commissioners for
executing the office of Lord High Admiral of Great
Britain and Ireland, &c.

MY LORDS,

I HAVE already given in to this honourable board
those observations I had to the East Indies, Guinea, and
West Indies, but not those I had in a second voyage to
the West Indies, nor from England to the Mediterranean.
Dr. HALLEY has perused them, and recommended them
to the public. And as nothing of this nature has ever
yet appeared, to my knowledge, I hope they may be of
use for ships bound to Guinea, West Indies, Mediter-
ranean, and East Indies: and as I think that all things of
this nature belong to this honourable board, especially
by me who have served as school-master many years in
the royal navy; therefore I most humbly dedicate them
to your lordships, and am, with all due respect,

YOUR LORDSHIPS

most humble and most obedient servant,

Jan. 30, 1738.

R^t. DOUGLASS.

THE variation of the compass in a voyage to, in, and from, the East Indies, in the years 1721, 22, 23, and part of 24, from the Island Madeira, on board the *Lyon*, THOMAS MATHEWS, Esq. Commodore. Likewise the variation to Guinea, from the Isle of Mayo to Cape de Verd, along the Coast to Widah, Island St. Thomas, Cape Lopez; back again to the Coast of Guinea; then to the Island St. Thomas; from thence to Barbados, St. Christophers, Jamaica, Porto Bello, along the Coast to Cartagena; then to Jamaica through the Gulph of Florrida, as far as $35^{\circ} 15'$ North, and West longitude from London $61^{\circ} 56'$; back again to Antegoa; from thence to England, in the years 1725, 26, and part of 27, on board the *Kinsale*, RICHARD GIRLINGTON, Esq. Commander. Some more to the West Indies, on board the *Lark*, JOHN GRAY, Esq. Commander, in the years 1727 and 172 $\frac{7}{8}$. More from England to the Mediterranean, as far as Leghorn, on board the *Dreadnought*, ALEXANDER GEDDES, Esq. Commander, in the years 1730 and 31. More in the Mediterranean, as far as Tripoly in Barbary, on board the *Hector*, the hon. Sir ROGER BUTLER, Commander in the years 1733, 34, and 35,

Note, the first column is the year and day of the month; the second and sixth, the variation; the third and seventh, the number of observations; the fourth, fifth, eighth, and ninth, the latitude and longitude at the time of observation. The longitude for the most part reckoned

reckoned from the meridian of London; if otherwise, it is taken notice of in the margin. When there are more observations than one, that set down is the mean. When in sight of land, I have set down its bearings and distance in the margin. E. stands for East, and W. for West variation.

We observed with 3 compasses outward-bound to the East Indies. The master of the ship managed one; the pilot his own; and I had the other, which I kept the whole voyage. We had but 2 homeward-bound; they seldom differed $\frac{1}{2}$ a degree, and not often that: they were taken with all the exactness possible. I have set down the latitude and longitude of most of the places I went from, and the latitude and longitude I made the place ly in (that I was bound to) by my reckoning.

The VARIATION of the COMPASS to, in, and from, the East Indies, on board the Lyon, THOMAS MATHEWS, esquire, Commodore.

1721 Mar.	Var. P. M.	N ^o of Obs.	Lat.	Long ^l .	Var. A. M.	N ^o of Obs.	Lat.	Long ^{de} .	
21	6 28W	2	29 20N	18 53W from London					Lat. of the W ^e end of Madeira 32 25 N. Long ^{de} . W ^e from London 17 42.
24	4 30	3	24 36	19 8					Bound for the island St. Jago.
25	3 36	2	23 0	19 36					
26	3 19	2	21 0	20 30					
28	2 25	2	16 40	22 38					
Apr. 8	2 30	1	14 15	23 26					Lat. of Port de Praya bay on St. Jago 15 10 N. obs ^d . Long ^{de} . W ^e from London 23 42 25 I have taken it. Bound for Cape Bon Espe- ranza.
9	2 29	2	11 40	22 13					
10	2 21	4	9 44	21 32					
11	2 4	1	8 17	21 33					
12					2 14W	1	6 28N	20 42W	
13	2 11	2	5 40	20 37					
14	1 30	1	4 16	20 26					
15	1 20	2	3 30	20 30					
17	0 33	2	2 10	21 8	0 0	1	1 22	21 15	
18					0 40E	2	0 7	21 29	
20	0 22E	1	1 20S	21 25					

1721 Apr.	Var ⁿ . P.M.	No ^o of Obs	Lat.	Long ^d .	Var ⁿ . A.M.	No ^o of Obs	Lat.	Long ^d .	
21	0 38W	1	1 46S	21 5W					
22	1 13	2	1 48	21 0					
May 2					2 11E	2	14 10S	25 14W	
3					3 0	1	16 0	26 42	
4	3 42E	1	16 50	27 16	3 26	3	18 0	28 2	
5	4 20	3	19 0	28 22	5 15	2	20 0	28 38	
6	4 20	2	21 10	28 22	5 18	2	22 38	27 56	
7	3 56	2	23 10	27 20	4 25	2	24 30	26 27W	Bound for Cape Bon Esperanza.
9					3 0	1	26 35	23 31	
10	2 50	2	26 56	22 15					
11	1 56	2	27 7	21 26					
12	1 33	2	27 36	20 17					
13	1 8	2	29 10	17 0	1 0W	2	29 40	14 12	
15	1 31W	2	30 10	11 27					
17	3 48	3	32 40	6 20	5 23	3	33 12	4 12	
19					7 50	3	34 2	2 30E	
21	9 52	5	34 20	7 10E					
24	13 24	2	33 32	14 40	13 22	3	33 58	15 20	
25					14 12W	3	34 9	17 4	

1721 May	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^{de} .	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^{de} .	
26									Ancl ^d in Table Bay, at Cape Bon Esperanza. By my reckoning I make the Table Land to ly in 18 10 E. from London.
June 16	25W	11							Taken as we lay at anchor in Table Bay. I judge there is something draws the compaſs.
14	14 25	2							Cape Bon Esperanza S. 5 or 6 lgs.
18	19 0	3	35 20S	28 53E	20 10W	3	35 16S	30 10E	Lat. of the Cape 35 25 S; Long ^{de} . E. from London 17 25.
19	20 45	4	35 14	31 12					Bound for Madagascar.
20					21 13	3	34 0	34 10	
22					22 35	3	32 2	35 38	
24					22 45	2	30 51	36 10	
25					22 40	3	30 39	36 17	
26	23 3	2	30 30	35 47	22 51	2	29 53S	36 26	
27	22 32	1	29 40	36 2					
28					22 24	2	27 33	37 12	For Madagascar.
29	23 43	2	26 38	37 34					
July 3	23 51	3	24 14	45 30					
4	24 17	4							St. Auguffin's Bay on Madagascar E. $\frac{1}{2}$ S. 5 or 6 lgs.

1721 July	Var. ⁿ . P.M.	Nr. of Obs.	Lat.	Long ^{de} .	Var. ⁿ . A.M.	Nr. of Obs.	Lat.	Long ^{de} .	
	23 18W	8							Taken as we lay at anchor in St. Augustin's Bay on Madagascar. I make St. Augustin's Bay on Madagascar to lye in 23 30 S. pr obs ⁿ . Long ^{de} E ^t . from London, by my reckoning, 46 21.
15	23 27.	2	21 40S	45 36E	22 35W	3	21 10S	45 40E	The nearer to the land the less var ⁿ .
16	23 51	2	20 47	45 57					
17	24 25	4	20 22	46 23					I take the land, called Westminster Hall near St. Augustin's, to lye in 23 38 S. and E ^t . long ^{de} from London, 46 45
19	21 44	3	20 20						
20	22 13	4	19 48	46 8					Lat: of the mouth of Timery river 20 20 obs ⁿ . Long ^{de} E ^t . from London, by my reckoning, 46 40.
21	22 22	1	18 17	45 18					Bound for the Island Joanna.
22	21 25	2	17 15	45 18	21 20	1	16 25	45 6	
24	21 33	3	15 0	45 26					
25	21 16	3	13 35	46 29					
29	20 33	4							In Joanna Bay.
Aug. 2	19 12	3							Center of the Island Comaro, S.W. by W. 4 or 5 lgs.
3	19 44	5			19 4	3			Center of the Island Comaro S.S.W. 10 lgs. Center of Comaro S. $\frac{1}{2}$ W. 18 lgs.

A g.	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^d .	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^d .	
4					19 2W	1	10 18S	44 57L	Mean of the evening and morning. Lat. of the center of Comaro 11 44 S. long ^d . E. from London 45 17.
5					18 27	2	9 34	44 58	
6					18 23	2	8 28	45 10	Bound for Cape Gardafoy near the Red Sea.
7	18 33W	4	7 36S	45 24E	17 20	2	6 28	45 36	
9					16 17	2	2 2	47 55	
12	13 48	1	3 5N	51 17					
13	13 19	3	5 25	51 41					
14	11 39	2	8 45	51 11	11 14	1	9 10N	51 30	P.M. Nearest land on the Ethiopian Coast W.N.W. 3 lgs. A.M. Nearest land N.W. by N. 4 lgs.; very cold.
15					11 0	3	10 6	51 54	Nearest land ditto Coast N. by W. 4 lgs.
16					11 11	3			Mount Felix at the mouth of the Red Sea W. $\frac{1}{2}$ N. 5 or 6 lgs. I make Cape Gardafoy, at the mouth of the Red Sea, to ly in 11 40 N. and E ^t long ^d . from London 52 30.
18	11 49	4	12 36	53 10					I take Cape Gardafoy to ly in 11 45 N. and E ^t long ^d from London 52 50.
19	11 0	2	13 10	55 37					Bound for Bombay.
20	9 42	2	14 48	58 45					

Aug. 1721	Var. P.M.	N. of Obs.	Lat.	Long ^d .	Var. A.M.	N. of Obs.	Lat.	Long ^d .	
21	8 28W	2	16 14N	61 37E					
23	7 7	5	18 20	65 35					
24					5 35	2	19 16	68 10	
27	5 12	3							The Low Land to the Sord of Valentine's Peak E. 3 lgs. near Bombay.
Sept 12	5 16	2							Taken without the W ^e gate on the Island Bombay.
	5 13	3							Taken aboard.
Oct. 5	27	3							Taken in the road by Choul; this is about 10 or 12 lgs. to the Sord of Bombay.
18	5 19	1							Island Calaba E.S.E. 2 lgs. This island is about 8 lgs. to the Sord of Bombay.
172 $\frac{1}{2}$ Feb. 1	4 58	2	17 0						In fight of Rogipore Island. Bound for Goa.
4	5 49	3							Alguarda by Goa S.E. 3 or 4 lgs.
5	5 25	3							Cape Ramas near Carwar, S.E. by E. $\frac{1}{2}$ E. 3 or 4 lgs.
7	5 40	2							The W ^e rmoff point of Car- war Bay N. by W. 7 or 8 lgs.
9	5 24	2							The W ^e rmoff rocks near Manqulor N. $\frac{1}{2}$ W. 3 lgs. This place lyes to the Sord of Baffore, about 9 lgs.

172 $\frac{1}{2}$ Feb.	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^d e.	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^d e.	
10 11	5 35W	8							At an anchor in Manqulore Road.
13	4 21	8							In Tellicherry Road.
18	4 5	3							In Calicut Road.
19	3 34	4							Cochin Fort N. by E. $\frac{1}{2}$ E. 4 lgs.
22	4 26	3	6 38N	77 56E					I take Anjanga to ly in 8 48N. and E. long ^d e. from London, 77 12.
23					4 0W	6	5 24N	78 24E	Bound for the Island Mauritius.
24	3 24	4	4 48	78 41					
25					3 9	2	3 55	78 38	
26	3 36	5	3 24	78 34					
27	3 3	2			3 8	1			At equal altitudes, having run but 7 mile. Mean of evening and morning.
28	2 53	3	3 16	78 4	3 7	2	3 13	78 10	
Mar. 1	2 35	2	3 5	78 24	2 48	3	2 48	78 34	
2	2 16	2	2 28	78 40	3 0	3	2 0	78 47	
3	2 20	5	1 28	78 53	3 4	3	0 50	79 0	
4	2 14	4	0 28	79 9	2 8	2	0 14S	79 14	
5	1 36	2	0 54S	79 20	2 10	3	1 10	79 24	
6	2 2	2	1 20	79 30	2 16	3	1 30	79 40	

1722 Mar	Var. P.M.	Nr. Obs.	Lat.	Long ^r .	Var. A.M.	Nr. Obs.	Lat.	Long ^{de} .	
7	1 48W	2	1 52S	79 8E					
8	2 28	2	2 45	79 36	2 46W	2	2 58S	79 36E	
9					3 6	3	4 12	79 48	
10	2 41	3	4 46	79 57	3 9	2	5 10	80 5	
11					3 24	1	5 36	80 22	
12					3 14	1	5 27	80 30	
13	2 54	2	5 27S	80 22	3 0	1	5 55	80 10	
14	3 15	2	6 26	80 2	3 28	1	7 2	79 45	
16	3 42	2	9 8	79 7					
17					5 20	2	11 36	78 30	
18	4 56	1	12 16	78 20	5 12	1	13 0	77 56	
19	5 8	2	13 50	77 20	6 15	2	15 0	76 24	
20	6 22	2	15 50	75 32	7 12	2	16 50	74 30	
21	7 26	2	17 40	73 22					
23					14 27	4	20 20	68 3	I can give no reason that the variation has risen so much. 2° 40 difference lat. and 4.19 difference of long ^{de} . The var ⁿ . has altered 7° 1'.
25					16 27	3	23 35	63 24	
26					17 44	1	20 41	61 6	
1722 27	17 37	4	20 42	60 57	18 47	4	20 35	59 34	

1722 Mar.	Var. ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^{de} .	Var. ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^{de} .	
28									Between 4 and 5 P. M. saw a round island, which lies near the island Mauritius. I make this island to ly in the long ^{de} . of 58 42 E. from London. I judge that this island and Don Mascareen ought to be laid down 3 degrees more to the East than most our books lays them in, for we often tryd the current.
Apr. 1722	18 46W	5							Taken at Peter Butts Bay, and this is about 4 mile to the Nord of N.W. harbour on Mauritius.
5	18 22	1			18 56W	1			The largest of the 2 round islands near Mauritius S.W. $\frac{1}{2}$ W. 4 l. A.M. d ^e island W. by S. $\frac{1}{2}$ S. 4 lgs.
6					19 7	1			D ^e island 8 or 9 lgs.
7					19 45	3			D ^e island N by $\frac{1}{2}$ W. 12 lgs. Bound for the Island Don-Mascareen.
8					19 49	2			The SE pt of Don Mascareen W.S.W. 6 or 7 lgs.
10	19 44	1							In St. Paul's Bay at Don-Mascareen.
13	19 49	2	19 12S	52 11E					I take St. Paul's Bay to ly in 20 58 S ^o . and E. long ^{de} . from London 54 10. Bound for the Island St. Mary by Madagascar.
18	19 53	1							Charrock Point on Madagafcar S. by W. 1 m. Long ^{de} . of the N ^o . end of St. Mary's 48 41, by my reckoning.

Apr. 1722	Var. P.M.	N 190 30 12	Lat.	Long ^{le} .	Var. A.M.	N 190 30 12	Lat.	Long ^l .	
19					19 52W	1			At Chanoek Point on Madagascari.
22	19 25W	2				2			't anchor at the mouth of St. Mary's harbour.
May 2	18 30	2	13 34S	52 35E					Bound for Cape Amber, which is the N. E. pt. of Madagascari.
3	19 1	5							Cape St. Sebastian on the No end Madagascari, S.W. $\frac{1}{2}$ S. 4 or 5 lgs.
4	18 36	3							Cape Sebastian, E. by N. 9 lgs.
5	19 48	3	13 0		20 12	1	13 8S		The great island Noiffe, E. $\frac{1}{2}$ S. 5 lgs.
6	19 45	3							The Earmost of the Isl ^d Noiffe, E. by N. 6 lgs.
7	19 5	3	13 34						Do Island E.N.E. 10 or 11 lgs.
8	19 20	3	13 56						We are to the Soid of Maringambo Harbour.
9					20 16	3	14 51		To the Nord of Soundjee point.
10					20 24	2	15 21		Near Maringambo Harbour. Mean of the evening and morning.
11	21 18	4							Mean of the morning and evening. At, an anchor in the outer bay of Manigaro.
14	21 14	4							Near New Maffeleage. Mean of the evening and morning.

1722 May	Var. ⁿ . P.M.	N Joanna	Lat.	Long ^{de} .	Var. ⁿ . A.M.	N Joanna	Lat.	Long ^{de} .	
15	21 37W	2							Jamgomy Island S. by W. 5 lgs. Going back to Manigaro Harbour.
16	20 51	4							Near Manigaro Harbour.
18 to 21	21 22	7							At an anchor in the inner bay of Manigaro.
June 7	21 43	1			21 14W	2	14 37	46 40E	Mackamoy Isl ^d S.W. by S. 7 lgs. Bound for the Island Joanna. Lat. of the N.W. part of Ma- nigaro Harb. 15 45 S ^o obs ⁿ . Long ^{de} . E. from London 47 26.
8	21 12	2			20 24	1			Saw Valentine's Peak on the Island Mayo a N.NE. 14 lgs. Long ^{de} . 45 46 E. Valentine's Peak N. by E. 6 leagues.
12	20 39	2							At an anchor in Joanna Bay, and with the same compass I observed here in June last. By another compass.
	20 33	2							
18	19 18	3	9 54S	45 34E	18 54	3	8 47	45 23	Lat. of the N.E. part of Jo- anna 12 2 S. Long ^{de} . E. from London 46 0, as I take it.
19	18 11	3	7 48	45 14					Bound to Fort St. George by the N ^o . end of the Maldivce Islands.
20	16 48	1	5 24	45 0					
21	16 15	2	2 26	45 56					
22	15 34	3	0 35	48 21					
23	15 4	2	0 18N	51 4					

1722 June	Var ⁿ . P. M.	N ^o of Obs.	Lat.	Long ^d e.	Var ⁿ . A. M.	N ^o of Obs.	Lat.	Long ^d e.	
24	13 59W	2	2 10N	54 10E					
25	12 43	3	3 16	57 2					
27	10 38	2	5 36	62 4					
29	9 11	2	8 34	66 45					
30	7 6	5	7 59	69 50					
July 1	7 8	4	7 53	71 44	-				
2	7 6	3	7 46	72 16	6 45W	1	7 42N	72 35E	
4	5 54	5	7 15	74 6	5 32	2	7 13	74 40	
5	4 56	3	7 10	75 22	5 0	1	7 0	76 10	
6	4 7	4			4 25	1			At equal altitudes evening and morning. In sight of the N ^o . end of the Maldives. I make the N ^o .most of the Ma'divee Islands to ly in 7 12 N. by a good obs ⁿ . and I make it ly in 77 15 E. from London. A fair wind all the way; but this is about 4° to the E ^{ard} of what our books lays it in; and its my opinion, that Maucitius and Dou-Mastareen, and the N ^o . end of Maldives ought to be laid down about 3° further to the E ^{ard} than they are.
7					4 43	3	7 5	0 49E ^t from the N ^o .most Maldi- vees.	
9	2 52	5	5 38	4 12E from the N ^o .most of the Maldivee Islands.					

1722 July	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^d .	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^d .	
10					2 13W	3	5 49N	7 43E from the N ^o most of the Maldi- vecs	A little after observ ⁿ . saw the Island of Zeloan. Note, I have made 7 56 long ^d be- tween the N ^o end of the Maldivees and the land cal- led the Elephant on Zeloan.
11					2 12	1			Fryers Hood on the S.E. part of Zeloan W.S.W. 8 lgs.
12	2 15W	1	8 30N		2 36	2	10 2	10 48W from Fry- ers Hood on Zeloan.	
July	2 52	9							At Fort St. George or Maderafs.
29	1 42	2	16 20	3 38E from Fort St. George.	1 32	1	17 0	4 16E	Bound for Balafore. The nearer the land I find the less variation.
30	1 38	1	18 3	4 41E from Fort St. George.					Note, I have made 5° 5' long ^d . between Fort St. George and Jackernat Pa- goda.
Aug. 7ber 8ber	3 4	26							At an anchor before the mouth of Rogues river; and, as I was informed, this is about 4 lgs. from the mouth of the Ganges.
28	3 33	2							Cape Palmitras near Palafore W. $\frac{1}{2}$ N. 4 lgs. Bound for Surrat.
29	3 0	1	18 56	0 9W from Cape Palmitras.					
31	3 17	2	14 50	2 5W from ditto.					The compasses new touched.

1722 9bre	Var. ⁿ . P.M.	N ^o of Obs	Lat.	Long. ^{de} .	Var. ⁿ . A.M.	N ^o of Obs	Lat.	Long. ^{de} .	
1	2 14 W	1	12 53 N	2 54 W	2 2 W	2	12 ON	3 17 W ^t from Pal- mitas.	
2	2 48	2	11 12	3 35					
4	2 21	2							Fryers Hood on Zeloan SW $\frac{1}{2}$ W 8 lgs. No ^t , I have made 4 ^o 38' long ^{de} . between Cape Palmitas and Fryers Hood on Zeloan.
5	2 36	1	5 44	0 23 W from Fry- ers Hood.					
11	4 11	3	9 14						Near Porcat, a Dutch factory on the coast of Malabar.
12	4 13	3							The ships in Cochin road N. by E. 4 or 5 lgs.
13	3 34	3							Cochin Fort E.N.E. 2 lgs.
14	3 14	4							We are about midway between Panfan Factory and Calicut.
15	4 9	3							In Calicut Road.
16	3 44	3							Sacrifice Rock S.S.W. $\frac{1}{2}$ W. 4 mile. This rock is about 4 or 5 lgs. to the S ^o of Tellicherry.
19	4 4	3							Tellicherry Fort S. E. by E. 5 lgs.
21	5 20	3							Saw Pidgeon Island from the mast-head N. by W. 10 lgs. Mallapy Hill E.N.E.
22	4 43	2							Pidgeon Island S. by E. 6 lgs.

1722 ber	Var ⁿ . P. M.	N ^r of Obs.	Lat.	Long ^{de} .	Var ⁿ . A. M.	N ^r of Obs.	Lat.	Long ^{de} .	
23	5 4W	3							In Carwar Bay.
25	4 37	4							Carwar Bay S.E. by S. $\frac{1}{2}$ E. 4 lgs.
26 27 28	4 57	8							In Goa Road.
30	4 56	2	16 6N						About 8 lgs. to the Nord of Vingola Rocks.
xber. 1	4 47	2	16 24						The mouth of Careptam Harbour E. by S. 3 mile.
2	4 50	2	16 50						Cape Dobs N.N.E. 5 lgs.
4	4 43	2	17 45						Sevendruke N.E. by E. 3 lgs. This is a rock fortified by Angria, and is about 4 lgs. to the Nord of Setra River.
5	4 50	3	17 55						Nearest land E.N.E. 6 lgs.
6	5 16	3							Island Canary near Bombay N. $\frac{1}{2}$ E. 7 lgs.
9	5 28	2							The land called the Neats Tongue E. by S. 8 lgs. Bound for Sumat.
10	5 36	1	19 37						Nearest land E. 5 lgs. Between Bombay and St. Johns.
11	5 27	3							The high land of St. Johns S.S.E. 5 lgs. from the nearest land.
12 to 20	5 50	17							In Surra Road.

1722 xber.	Var ⁿ . P.M.	N. MO	Lat.	Long ^{de} .	Var ⁿ . A.M.	N. MO	Lat.	Long ^{de} .	
22	5 23W	2							Demaen E S.E. 4 or 5 lgs. Bound for Bombay
23	5 26	2							The Neats Tongue S.E. by E. 7 lgs.
29 to 31	5 7	6							At Bombay.
Feb. 5 to 9	5 59	7							In Surrat Road.
1722 ² / ₃ 14	5 30	1							We are about 7 or 8 lgs. to the Sord of Dundee Rogipore. Bound for Goa.
15	5 5	1	16 14N						We are a little to the Sord of Careptam Harbour.
17 to 19	5 13	5							In Goa Road.
24	5 8	3							In Carwar Bay.
25	5 5	1							The Spermoft part of Carwar Bay N. $\frac{1}{2}$ W. 5 lgs.
27 28 Mar. 1	5 5	8							In Mangulore Road.
3 4	4 9	3							In Tellecherry Road.
7	4 22	2	11 18	75 30E from London.					I take Sacrifice Rock to ly in 11 30 N. and E. long ^{de} from London 75 38.
8	4 51	3	10 35	74 32	.				Bound for the Island Saccatra.

1723 Mar.	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^d .	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^d .	
9	5 11 W	2	9 54 N	72 55 E					Island Quilpenna 100 N. obs ⁿ . Long ^d . E ^t . from London 73 20.
10	5 24	1	9 43	72 25					
11	5 46	1	10 6	72 21					
13	6 3	3	9 54	71 32					
14	5 43	2	9 46	71 17					
15	5 44	1	10 0	71 17					
16	5 38	1	9 38	70 55					
19	7 14	2	9 12	68 5	7 0 W	1	9 25 N	67 29 E from London	
20	7 11	2	9 25	67 10	7 58	1	9 40	66 40	
21	8 30	2	9 45	66 9	8 22	1	9 54	65 28	
22	8 42	1	10 0	64 55					
23	9 16	2	10 37	63 53	8 33	2	11 12	63 27	
24	9 28	3	11 20	63 0	9 24	2	11 36	62 30	
25	9 54	4	11 40	62 3	9 45	2	12 0	61 20	
1723 26	10 12	2	12 10	60 53	9 48	1	12 20	60 11	
7					9 41	2	1	59 21	
28	*				9 48	2	12 21	59 30	

1723 Mar	Var ⁿ . P.M.	N ^o of Obs ^t	Lat.	Long th .	Var ⁿ . A.M.	N ^o of Obs ^t	Lat.	Long th .	
29	9 16W	1	12 30N	59 30E	9 18W	4	12 22N	59 20E from London.	
30	9 29	2	12 28	59 17	10 8	1	12 23	59 13	
31	9 48	1	12 32	59 12					
Apr. 1					10 6	1	12 45	58 19	
2	10 40	1	12 50	57 40	10 39	2	12 50	56 57	
3	10 45	2	12 48	56 0	11 26	1	12 44	54 45	
4	11 16	2	12 36		11 33	1			The Et. end of the island Sac- catra W. $\frac{1}{2}$ N. 9 lgs.
5	11 36	1							Et. end of Saccatra S.E. by S. 5 lgs. Note, I have made 21° 8' of long th . between the island Qualpenna and the Et. end of Saccatra; but as those places is laid down in books, I should not have made so much by about 2°. Lat. of the E ^m ost point of Sac- catra 12 20 N. obl.
8	11 38	4							At an anchor before the town of Saccatra, which stands on the N ^o . side of the island.
10	12 4	1							Lat. of the town 12 20 N. obs ⁿ . The W ^t . end of Saccatra S.W. $\frac{1}{2}$ W. 5 lgs.
11	11 54	2							The mean between the morn- ing and evening. At equal altitudes morning and evening.
12	12 0								

the Variation of the Compass.

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1723 Apr.	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^d .	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^d .	
12	12 43W	2							The E ^t . end of the island Abdehura S. by E. 10 lgs.
13	12 18	2							Mount Felix E ^t S ^o erly 5 lgs.
14	12 16	2	11 36N	1 0W from Mount Felix.					
15	12 58	1	11 25	2 15	12 32W	1	11 46N	3 6W from Mount Felix.	
16					13 4	2	12 36	4 38W from D ^o .	
17	12 37	3	12 50	4 48W from D ^o .					
18	13 50	2			13 40	2	12 36	0 56W from Cape Aden.	Cape Aden N.E. $\frac{1}{2}$ N. 5 lgs. This Cape ought to be laid down a degree more to the W ^{ard} than it is.
19	14 20	1							Taken in the narrow straits of Babelmandel.
20 to 27	13 34	7							In Mocha Road in the Red Sea.
May 14	14 1	1							Zee Hill near the narrow straits of Babelmandell E. by S. $\frac{1}{2}$ S. 2 lgs.
15	14 8	1							The middle of the narrow straits of Babelmandell S. by E. 2 lgs.

1723 May	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^d .	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^d .	
19	13 54W	1							The highest land of Cape Aden N.W. $\frac{1}{2}$ W. 5 lgs.
20	13 42	1							Cape Aden W.N.W. $\frac{1}{2}$ N. 9 lgs.
21	13 25	2	12 32N	0 30E from Cape Aden.					
22	13 10	1	12 16	1 0E from D ^o .					
23	13 6	1	11 56	2 4E from D ^o .					
24	13 23	2	11 58	2 30					
25	12 39	2	12 4	3 12					
27	13 3	3							Within 5 or 6 lgs. of the Ethiopian shoar.
28	12 0	2							About 3 or 4 lgs. from Mount Felix. Note, I made 6° 14' long ^d from Mount Felix to Cape Aden, and in coming back 6° 21.
29	12 34	2							Cape Gardafoy S.E. $\frac{1}{2}$ E. 5 lgs.
31	12 20	2							The town of Saccatra S.W. by W. 6 lgs.
June 4	8 26	1	9 50	66 50E from London.	*				I take the E ^t . end of the island Saccatra in 12 20 N. obs ⁿ . and E ^t . long ^d . from London 56 22.

1723 June	Var ⁿ . P. M.	N ^o of Obs.	Lat.	Long ^{de} .	Var ⁿ . A. M.	N ^o of Obs.	Lat.	Long ^{de} .	
5	7 34W	2	8 40N	69 14E					Bound for Fort St. David's on the Coast of Cormandel by the N ^o . end of the Maldivee Islands. Note, I have made 20° 38' long ^{de} . between the Et. end of the Island Sacca-tra and the N ^o rm ^o st of the Maldivee Islands; and this makes the N ^o rm ^o st island to ly in 77° 0 E. from London; and in sailing from the Island Joanna to this N ^o rm ^o st island I made it ly in 77 15. Note, we had a fair wind both voyages.
11	2 2	2	5 53	7 4E from the N ^o rm ^o st of the Maldiv- ee Islands.					In the morning saw the Island Zeloan to the W ^{ard} of Dondra-Head. of long ^{de} . between the N ^o rm ^o st of the Maldivee Islands and Dondra-head on the Island Zeloan 7° 46'. Fair wind all the way.
29	2 25	4							At Fort St. Davids. Mean of the evening and morning.
July 15 17	3 16	4							At Fort St. George or Made-rafs.
8ber 14	2 6	3			2 8W	1	5 23N		Dondra Head (N.E. $\frac{1}{2}$ E. 6 lgs.) on Zeloan. In sight of Zeloan.
15	2 46	1							The Dutch factory at Point de Gallee on Zeloan N.N.E. 2 lgs.
16	2 45	2							A land that appears like a hay-cock near d ^{ty} . de Gallee on Zeloan E. $\frac{1}{2}$ S ^o . About 6 lgs. from the nearest land.

1723 8ber.	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^d .	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^d .	
17	2 44W	1	7 20N	0 53W from Point de Gallec.	2 52W	1	7 54N	1 22W from Point de Gallec on Zuloan	Bound for Bombay.
18	2 51	3			2 48	3			The high land of Cape Cam- eroon W. 10 lgs. Cape Cameroon W. by N. $\frac{1}{2}$ N. 10 lgs.
19	2 50	1							The high land over Cape Cameroon, W.N.W. 6 lgs.
20	3 9	1							Cape Comaroon N.N.W. $\frac{1}{2}$ W. 3 or 4 lgs.
21	2 58	2							Do. Cape Et. 4 or 5 lgs.
9ber 1	5 8	2							Manqulore E. by N. $\frac{1}{2}$ N. 3 or 4 lgs. On the Malabar Coast.
2	5 13	3							Baffalore Hill N.E. We are about 5 lgs. from the nearest land.
3	4 48	2							The Spermot part of Carwar Bay N. 6 lgs.
4	5 48	4							Cape Ramas E. by N. 2 lgs. Here there is something that draws the compass.
6	5 3	4							At Goa.
9	5 26	3							Rogipore N.E. $\frac{1}{2}$ E. 5 or 6 lgs.
10	5 28	2							Cape Dobbs E. by N. 2 lgs.
12	5 25	3							Island Canary near Bombay N. by E. $\frac{1}{2}$ E. 6 lgs.

1723 gber	Var ⁿ . P.M.	N ^o of Obs ^r	Lat.	Long th .	Var ⁿ . A.M.	N ^o of Obs ^r	Lat.	Long th .	
13	5 10W	2							Going into Bombay.
xber 17 18	5 48	4							In Surrat Road.
xber	5 22	1							At Bombay.
1723 ¹ Jan.	5 41	3							At Goa.
5	5 32	2							In Carwar Bay.
6	5 33	2							Pidgeon Iſland S.E. by S. 3 lgs
9	2 46	1							Mount Dilla near Tellecherry E N.E. 2 mile. Here its plain that being near the land the compaſs is drawn.
10 11 12	4 34	6							In Tellecherry Road.
16	3 41	1							Note, I have all along obſerved that the variation decreaſes after you paſs Tellecherry, till you are 4 or 5 lgs. to the Sord. of Cochin; then increaſes again till you are to the Sord of Anjanga.
18 19	3 26	2							In Cochin Road.
21	4 16	1							Cochin Road N.E. $\frac{1}{2}$ E. 4 or 5 lgs.
22	4 38	1	9 20N						About midway between Co- chin and Anjanga.

1724 Mar.	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^d .	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^d .	
12	18 43W	4	36 40S	22 50E	18 56W	2	36 10S	21 38E	
13	18 16	2	35 57	20 47					
14	17 3	6	36 8	18 24	16 53	3	36 6	18 4	
15	16 23	3	36 6	17 53					
16	16 12	4							Cape Falfo near Cape Bon- Esperanza N.W. 6 lgs.
21 to 29	16 27	8							In Table Bay at Cape Bon- Esperanza. The fort S.S.W. 2 mile.
26 to 29	16 18	5							By another compass.
31	16 15	2							Going out of Table Bay. The fort S.S.W. $\frac{1}{2}$ W. 6 or 7 mile.
Apr. 1	15 8	2	32 47	15 36					Lat. of the Table land 54 16 S°. Long ^d E. from Lon- don 17 24.
2	15 15	2	32 44	14 50					
3	14 59	1	32 0	14 36	14 51	2	31 26	14 27	
4	14 33	3	31 20	14 34					
5					14 27	3	29 31	13 55	
6	14 30	6	29 0	13 34					Bound for St. Helena.
7	13 0	2	27 15	11 22					
9	11 40	2	23 8	6 29	12 4	4	22 20	5 37	

the Variation of the Compaſs.

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1724 Apr.	Var ⁿ . P.M.	N ^o of Obs ^r	Lat.	Long ^o .	Var ⁿ . A.M.	N ^o of Obs ^r	Lat.	Long ^h .	
10	12 2W	6	21 56S	5 0E					
13	10 47	2	17 54	1 0	10 10W	2	16 55S	0 8W	
14					9 30	2	16 4	2 48	
15					8 6	1	15 54	4 47	
16	7 18	3							The center of the island Wt. N ^o erly 6 or 7 lgs. I make St. Helena to ly in 6 16 W. from London, by my reckoning.
17 to 26	7 42	5							In St. Helena Road.
24	7 26	2							By another compafs.
30	7 22	1							St. Helena Fort S.E. 3 lgs.
May 1	7 30	1	14 36	7 32W from London.					I take St. Helena Fort to ly in 16 0 S ^o . and Wt. Long ^{de} from London 6 5.
2	6 54	4	13 26	8 36					Bound for England.
4	5 44	4	10 36	11 6					
5	5 27	6	9 4	12 24					
6	5 1	2	7 18	13 55					
7	4 37	2	5 30	15 21					
8	4 46	1	3 46	16 17					
9	4 16	1	2 6	16 50	3 48	1	1 14	17 8	
11	3 40	1	1 0N	17 40	13 39		2 9N	17 49	

1724 May	Var. P.M.	N. 190 30	Lat.	Long ^{d.}	Var. A.M.	N. 190 30	Lat.	Long ^{le.}	
12	3 34W	1	3 8N	17 57W	2 56W	2	4 6N	18 5W	
16					3 29	2	9 28	18 57	
17	3 55	4	9 46	18 53					
18	4 26	1	10 20	9 31					Bound for England.
19					3 18	1	11 18	20 40W from London.	
20	3 55	3	11 39	21 13	3 38	2	11 30	21 46	
21	3 38	2	11 28	22 16					
23	2 26	1	12 0	23 59					
24	2 45	3	13 8	25 1					
25					2 15	2	14 30	26 29	
27	2 46	3	16 0	28 10					
28	2 8	5	17 23	29 17					
29	2 43	2	18 42	29 22					
30	3 26	4	19 58	31 14					
31	3 12	6	21 25	31 41					
June 1	2 48	6	22 25	32 15					
2					3 29	3	25 5	32 8	

1724 June	Var. P.M.	N ^o of Obs.	Lat.	Long ^d .	Var. A.M.	N ^o of Obs.	Lat.	Long ^d .
3	3 39W	3	25 44N	33 30W				
4	4 22	2	26 31	34 23				
5	5 8	5	27 18	35 5	4 37W	2	27 43N	35 29W
6	5 15	4	28 6	36 0				
7	5 30	3	28 56	36 22				
8	5 41	3	29 16	36 23				
9					5 42	1	30 24	36 40
10	6 10	5	30 46	36 52	5 58	2	31 14	37 15
12	6 57	4	32 26	38 13				
13	7 15	2	32 58	39 3				
15	7 19	4	34 30	40 30				
16	7 47	4	35 46	41 14				
17	8 29	4	37 0	41 39				
18	8 55	3	38 40	42 4				
19	8 21	5	39 33	40 37				
20	8 52	4	40 42	38 37				
21	9 16	3	41 43	36 5				
22	9 37	3	42 26	33 41				
23	9 38	2	43 34	31 7				

1724 June	Var. P.M.	N ^o of Obs ^r	Lat.	Long th .	Var. A.M.	N ^o of Obs ^r	Lat.	Long th .	
24	10 38W	6	44 16N	28 26W					
25	11 28	1	44 30	28 5					
29	11 14	2	45 26	27 22					
30	11 50	4	46 0	25 18					
July									
1	11 55	6	46 34	23 14					
2	11 51	1	46 54	21 54					
3	12 10	1	47 32	19 33					

Note, The Bill of Portland was the first land we made, and I am about $\frac{1}{2}$ a degree further to the E^tard then it is laid down in our books.

The VARIATION of the COMPASS in a voyage to Guinea, W^t. Indies, and back to England, in the Kinfale.

1725 8ber.	Var ⁿ . P. M.	N of O	Lat.	Long ^d .	Var ⁿ . A. M.	N of O	Lat.	Long ^d .	
5	3 32W	1							The Peak Hill on the Isle of Mayo S.S.E.; we are about a mile from the shore.
7 8	4 5	4							At anchor in Port de Prya Bay on the S.E. part of the Island St. Jago.
11	4 36	2	14 0N	20 0W ^t from London.					Bound for Cape de Verd, Lat. of Port de Prya Bay 15 10N. obs ^d . Long ^d . W ^t . from London 23 42.
12	5 4	2	13 33	18 36					
13	5 21	3	13 44	17 30					At noon saw Cape de Verd N.N.E. 3 lgs.
14	5 11	1	13 53						In sight of Gambia River.
15	6 29	2							Cape St. Marys near Gambia River S.W. 5 or 6 lgs.
16 21	6 19	8							At a anchor in the mouth of Gambia River.
25	4 36	1	11 6	16 12	4 59W	2	10 42N	15 54W	Lat. of Cape St. Marys 13 20 N. Long ^d . W ^t . from London 15 36.
26					4 54		10 18	15 20	Bound for Siera Leon.
29	4 18	1	9 52	14 54	3 50		9 28	14 36	
30	4 13	2	9 12	14 16					

1725 9 Oct	Val ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^{de} .	Val ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^{de} .	
1	3 49 W	2	9 16 N	14 10 W					
8 9	5 12	3							In St. John River I judge there is something draws the compass.
14	4 47	2	7 56	12 42					Lat of Bonas Islands 8 6 N. Long ^{de} W ^t . from London 12 10
15	4 52	1	7 59	12 48					Bound for Cape Mount
19	5 49	2	7 6	11 50					
20	6 20	2	6 56	11 9					
21					5 48 W	1	6 42 N	10 49 W	
22	5 24	2	6 42	10 49					By equal altitudes evening and morning
24	7 0	2			7 5	1			Cape Mount E S E 5 or 6 lgs. By equal altitudes evening and morning
25	6 58	2							Cape Mount E N E 2 mile.
27	7 15	2							Near the River St. John, and about 3 or 4 mile from the shore.
28					7 30	1			Near Great Bassa, and a little to the W ^d . of the River St. John.
29	7 28	2							Great Bassa N ^e . 3 lgs. 2 miles from the shore.
30	7 46	1							About 4 lgs to the W ^{ard} of Sector of Sestos Road.

1725 8ber	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^{de} .	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^{de} .	
4	7 30W	2							In Sefior Road.
5	7 15	2							The mouth of Sefior River N.E. by N. 4 lgs.
7	9 8	1							Setra Crue N. by W. 3 lgs.
11	9 38	2							About 5 lgs. to the W ^t ward of the river St. Andrews.
13	9 54	1							About 3 lgs. to the W ^t ward of Cape de la Hou.
15	10 58	1							River Casto N.W. by N. 3 mile.
18	11 0	1							The Village Tabo N.N.E. $\frac{1}{2}$ E. 3 or 4 mile.
20 22 24	11 37	5							At Axim.
Jan. 1	11 55	3			12 10W	1			At Cape Coast. At equal altitudes.
5	11 25	3							The English fort at Accara N.W. 2 mile.
7	12 0	1							A little to the W ^t ward of the River Volta.
8	12 17	2							The flag at Little Papa N. by E. $\frac{1}{2}$ E. 2 mile.
11	12 45	2							In Widah Road.
20	13 28	1	4 50	2 23E from London.					Lat. of Widah 6 40 N. Long ^{de} . E ^t . from London 2° 10'.

172 $\frac{1}{2}$ Jan.	Var ⁿ . P.M.	N. of Obs.	Lat.	Long ^{de} .	Var ⁿ . A.M.	N. of Obs.	Lat.	Long ^{de} .	
21	13 14W	1	3 42N	2 35E	13 30W	1	3 25N	2 32E ^t from London.	Bound for the Isl ^d St. Thomas.
23	13 54	3	2 16	3 30	13 50	1	2 2	3 40	
24	14 7	3	1 48	3 58					
25	14 19	1	1 31	4 53	14 40	2	1 32	5 15	
26	14 36	1	1 27	4 56					
31	14 48	1							At the Island St. Thomas. I make the Isl ^d Anna de Chary by St. Thomas to ly in 00 16 N. per obs ⁿ , and E ^t . long ^{de} . from London 6 16.
Feb. 8	14 44	1							The Island Anna de Chary near the Island St. Thomas S.E. by E. about 7 lgs.
10					14 56	1	1 16S	1 16E from the Island An- na-bona.	Bound for Cape Lopez.
11	14 35	1	1 10S	1 44E from An- na-bona.					
15					14 50	1	0 56	0 56W from Cape Lopez.	
16	14 30	1	1 4	1 17W from Cape Lopez.	15 1	2	1 9	1 43	
20	14 21	1	1 30	1 4W from An- na-bona.					Lat. of the Island Anna-bona 1 48 S ^o . obs ⁿ .

17 th Feb.	Var. P.M.	N of S	Lat.	Long.	L.M. /al ⁿ .	N of S	Lat.	Long ^{de} .	
22	13 10W	2	1 46E	2 11W from D ^o .	13 48W	1	1 38S	2 15W from A ⁿ - a-bon.	Bound for the Coast of Guinea again.
23	13 28	2	1 35	2 19	13 37	2	1 36	2 41	
24	13 46	1	1 39	3 10	13 50	1	1 25	3 35	
27	12 20	2	0 50	7 24	12 6	2	0 40	8 4	
28	12 21	2	0 20	8 18	12 25	1	0	8 35	
Mar 1	12 6	1	0 24N	8 55	12 20	1	1 10N	9 23	
2	12 6	1	1 34	9 42	11 44	2	1 50	9 57	
3	11 47	2	2 4	10 3					
5	11 30	3	4 16	11 21					
6	11 50	2							
7	11 40	1							equal A ⁿ tin des.
11	11 40	1							At Dickj's Cove.
14	11 27	2							Little Commendo N. by E. $\frac{1}{2}$ E. 4 lg.
15	11 46	2							At Cape Coast.
21	11 34	1							At Anhimaboc.
23 24	11 53	1							At Accra. Mean between the 3 last and this one.
28	12 37	2							At an anchor about $\frac{1}{2}$ lg. to the W ^{ward} of Little Papa.
29	12 43	2							At Little Papa.

1726 Apr.	Var. P.M.	N. of Obs.	Lat.	Long ^d .	Var. A.M.	N. of Obs.	Lat.	Long ^d .	
13 14 17	13 10W	4							In Widah Road.
22	13 0	1	5 18N	2 46E from London.					Lat. of Widah 6 48 N. obs ⁿ . Long ^d . Et. from London 2 10.
24	13 11	1	4 21	2 50					
30	14 35 14 29*	2							At the Island St. Thomas. "By equal altitudes evening and morning. Note, by the of long ^d . made from Widah this and the last time, I dif- fered but 4 minutes; so that I judge the Island St. Tho- mas is laid down almost 2 de- grees too far E ^t ly, and our passage from St. Thomas to Barbadoes confirms me in this opinion, having gained that passage in 33 days, and had a fair wind all the way.
May 19	13 44	1	0 4	3 48E from London.	13 44W	1	0 10S	2 48E from London.	Lat. of the Island Anna de Chares by St. Thom. 0 16N. Long ^d . Et. from London 6 16.
20	13 10	2	0 28S	1 51	12 42	1	0 46	0 39	Bound for Barbadoes.
21	12 34	1	1 2	0 13W from London.	11 38	1	1 18	1 33W from London.	
22	11 12	1	1 33	2 41					
23	10 4	1	1 45	5 15					
24	8 35	1	2 9	7 46	8 34	1	2 22	9 32	

1726 May	Var. ⁿ . P.M.	N ^o of Obs.	Lat.	Long. ^k .	Var. ⁿ . A.M.	N ^o of Obs.	Lat.	Long. ^{le} .	
25	6 54W	1	2 31S	10 44W	6 52W	1	2 36S	11 51W	
26	6 23	2	2 40	12 40					
27	4 53	2	2 38	14 30	4 24	2	2 29	15 34	
28	3 31	2	2 30	16 24	2 21	2	2 50	17 36	
29	2 25	1	2 52	18 39					
30					0 41	2	2 53	22 33	
31	0 5E	1	2 46	23 44	0 4E	2	2 27	25 1	
June 1	0 17W	2	2 12	26 6	0 16W	3	1 33	27 2	
2	0 16E	2	1 0	28 4	0 18E	3	0 19	29 7	
3	0 22	1	0 25N	29 40	0 19	1	1 7N	30 39	
4	0 36	2	1 38	31 18	0 26	1	2 20	31 55	
6					0 26	1	4 20	34 6	
7	0 34	1	4 42	4 13	0 37	2	5 0	34 16	
8	0 30	2	5 20	34 22W					Bound for Barbadoes.
9	0 14	1	6 47	35 33	0 10	1	7 4	37 0W ^r . from London.	
11					0 36	1	8 42	41 10	
13	0 50	1	10 4	44 52	1 32	1	10 48	46 25	
14	1 36	1	11 18	47 43	1 26	1	11 48	49 2	
15					2 22	1	12 26	51 6	

1726 June	Var ⁿ . P. M.	N ^o of Obs.	Lat.	Long ^{de} .	Var ⁿ . A. M.	N ^o of Obs.	Lat.	Long ^{le} .	
16	2 16 E	1	12 44 N	52 22 W	2 37 E	2	12 56 N	54 6 W	
17	3 0	1	13 5	55 28	3 36	1	13 12	57 3	Note, I make the center of Barbadoes to ly in 13 27 N. by obs ⁿ . and 56 17 W. long ^{de} . from London 58 17. Having a fair wind all the way, this makes me believe that the Island Anna de Charco; viz. 16 16 E. is near the truth.
26 27 28	4 24	3							In Carlisle Bay at Barbadoes.
29	3 26	1							Lambert's Point on the N. Wt. part of Barbadoes E. S. E. 5 lgs.
July 1	3. 22	1							The W ^{term} of part of Guadalupe N. E. $\frac{1}{2}$ N. 4 or 5 lgs.
2	4 10	1							At Backstar on the Island St. Christophers.
6	5 22	1	17 58	9 2 W from Palmetta Point on the Island St. Christophers.					Within 4 or 5 lgs. of Hispaniola, and a little to the W ^{ward} of Cape Zapazen on Hispaniola.
7	5 12	1	17 40	0 46 W from the Island Alovala near Cape Alouga on Hispaniola.					

1726 July	Var ⁿ . P. M.	N ^o of Obs ^r .	Lat.	Long ^{de} .	Var ⁿ . A. M.	N ^o of Obs ^r .	Lat.	Long ^{de} .	
9	5 3E	1	18 ON	0 46W ⁿ from D ^o .	5 12E	1			Saw the E ^t . end of Jamaica.
12	6 2	1	17 6	0 12E from Port- land Point on Jamaica					Bound for Porto Bell.
13					6 30	1	14 28N	0 38W from Port- land Point on Jamaica	
14	7 0	1	13 32	0 58W from D ^o .	7 8	1	12 27	1 25W from D ^o .	
15	7 16	1	11 32	1 52W from D ^o .	7 30	1	10 46	2 18W from D ^o .	
16					7 22	1	9 51	2 39W from D ^o .	
22					7 48	1			At an anchor without the Island called the Great Baf- timientos.
Aug. 7	7 24	1							Cape Cathivas near the Gulph of Darien S.E. by S. 7 lgs.
9	7 30	1	9 40	1 27E from Cape Cathivas.					
11	6 50	1							The land called Mother la Popenca Catagena S. by E. ½ E. 5 lgs.

1726 Aug.	Var. ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^{de} .	Var. ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^{de} .	
12	6 20E	1							Point Jamba near Castagna E.S.E. 3 lgs.
13	6 14	1	12 52N	0 42W ^t from Point Jamba.					Bound for Jamaica.
14	6 4	1	14 0	0 50W ^t from D ^o .					
15	5 42	1	15 18	1 3W ^t from D ^o .					
16					5 28E	2	17 6N	1 30W ^t from D ^o .	
7ber 12	4 31	3							At Port Royal in Jamaica.
8ber 12					4 49	1	18 40	1 53W ^t from Point Negril on Jamaica.	
13					4 41	1	19 54	0 57W ^t from the N.W. end of the Grand Commanis	
14					4 4	1	20 44	2 7W ^t from D ^o .	Bound through the Gulph of Florida.
17	4 7	2	22 53	0 16W ^t from Cape St. Anto- nio on the W ^t . end of the Island Cuba.					

1726 ber	Var ⁿ . P. M.	N ^o of Obs.	Lat.	Long ^{de} .	Var ⁿ . A. M.	N ^o of Obs.	Lat.	Long ^{de} .	
18	4 25E	2	22 31N	0 55E ^t from Cape Antonio.					
19					4 0E	1			The middle of the Table Land S. by E. $\frac{1}{2}$ E. 4 lgs. This Table Land is about 5 lgs. to the W ^t ward of the Ha- vanna.
23					4 24	1			The high round hill, called Pan de Mathaneas, a little to the E ^t ward of the bay of the ſame name on Cuba, bore S. W. by S. 10 or 12 lgs.
24	4 23	2	23 30	0 38E from Pan de Matha- neas.					
25	3 26	1							The land about Cape Florida N. by W. 6 lgs. A great ſwell.
26	3 5	1	25 12	81 20W from London.					In the Gulph. Lat. of Cape Florida 24 48 N. Long ^{de} . W ^t . from London 21 55
28					2 11	1	27 50N	81 30W from London.	A great ſwell.
30					0 56	1	29 35	80 40	Do. ſwell.
ber 3					2 14W	1	33 25	71 52	A very great ſea.
6					5 12	1	35 4	64 6	A great ſea.
8	5 8W	1	35 15	61 56					

1726 9her	Var ⁿ . P.M.	N of Ob ⁿ	Lat.	Long ⁿ .	Var ⁿ . A.M.	N of Ob ⁿ	Lat.	Long ⁿ .	
9	4 55W	1	35 ON	61 55W from London.					Going back to the Wt. Indies. Swell much abated.
10	5 6	1	33 14	61 14					
14	0 41	2	26 14	58 9	0 36W	1	25 48N	58 6W from London	
15	1 3	1	25 41	58 0	0 54	1	25 20	37 14	
16	4	1	24 48	56 38					
17	0 20	1	23 0	56 35					
18	0 32E	1	21 7	56 32	1 46E	1	19 54	56 21	
19					1 46	1	18 56	56 21	
20	1 43	1	18 32	56 22	2 25	1	17 34	56 17	
21	2 15	1	16 52	56 22	3 11	1	16 12	56 50	
22					3 26.	1	15 58	58 6	
23	3 27	1			3 40	1			Island Descado S.W. $\frac{1}{2}$ W. 5 lgs. I make Descado to to ly in 16 10 N. obs ⁿ . and W. long ^d . from London 59 5. The center of the island Ma- rigalant N.W. 5 lgs. Do- minico S.W. by S. 5 lgs.
29	4. 4	1							In sight of Antego.
1727 Apr. 14	4 28	1							The high land of the Island Nevis W. $\frac{1}{2}$ S. 7 lgs. The Island Rodunda S. by W. $\frac{1}{2}$ W. 7 lgs. The center of the Island Antego E. by S. $\frac{1}{2}$ S. 7 lgs.

1727 Apr.	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^d .	A.M. Var ⁿ .	N ^o of Obs.	Lat.	Long ^d .	
15	3 30E	2	18 36N	61 18W from London.	2 56E	1	19 20N	61 20W from London.	Lat. of the Island Nevis 17 10 N. Long ^d . W. from Lon- don 61 10.
17	3 2	4	19 50	61 14					Bound for England.
19	2 22	3	20 56	61 28					
22	0 51	3	25 20	61 24	1 7	3			
23	0 43	1	25 54	61 23	0 16	2	26 6	61 21	
24					0 15W	2	26 16	61 30	
25					0 41E	1	26 24	61 47	
26					0 18	3	26 34	61 48	Mean of the evening and morning.
27	0 35	1	27 44	61 44					
28					0 38W	4	28 36	60 52	
29	0 42W	2	28 46	60 54	0 48	2	29 6	60 50	
30	1 18	2	30 9	60 45	1 56	1	31 26	60 4	
May 1	2 32	1	32 28	59 33	3 16	1	32 54	59 16	
2	4 5	2	33 54	58 43					
6	9 5	2	40 28	55 42					
7	10 12	1	41 56	52 54					

1727 May	Var ⁿ . P. M.	N ^o of Obs.	Lat.	Long ^{de} .	Var ⁿ . A. M.	N ^o of Obs.	Lat.	Long ^d .	
9	11 54 W	2	43 56 N	48 41 W					This day founded having 35 fathoms fine brown sand; by this we judge we are on the great bank.
10	12 37	3	44 30	46 0					
12	14 20	1	46 12	39 18					A great sea.
13	14 0	1	46 51	35 15					Do. sea.
15	12 54	1	46 16	28 25					Do. sea.
17	11 39	2	46 10	23 53					Smooth water.
21	11 46	2	48 45	15 12					
26	13 24	2							In sight of Portland, dist. about 5 lgs.

Note, By my reckoning I made the Lizard to ly in 50 6 by judgement, and W^t. longitude from London 5 27; and as I have set down the lat. and long^{de}. at the time of obsⁿ. by this it may appear how far these observations may be relied on.

VARIATION of the COMPASS from Madera to the W^t. Indies,
Lark, Captain GRAY.

1727 xber	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^{de} .	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^{de} .	
30	6 58 W	3							In Madera Road.
Jan.	6 18	1	28 52 N	19 46 W ^t from London.					Lat. W ^t . end of Madera 32 25 N. Long ^{de} . W ^t . from London 17 42.
1727 18					2 16 E	1	16 19 N	51 5 W from London.	
19	2 14 L	1	16 29	52 8					
20	2 48	2	16 18	54 53	3 58	1	16 16	56 47	
21	3 58.	1	16 24	58 20					
22	3 40	2							Island Descado S.E. $\frac{1}{2}$ E. 8 lgs. N.W ^t . part of Guardalupa S.W. $\frac{1}{2}$ S. 6 lgs. N.E ^t . part of Antego N.W. by W. 8 lgs.
23	4 49	2							At the Island St. Christopher.
27	6 2	2							Island Altovalle near Hispani- ola N.E. by N. $\frac{1}{2}$ E. 5 lgs.
Feb. 19	7 37	3							The land called Mother La Pope, near Cartegena, S. by E. 10 or 12 lgs.

VARIATION of the COMPASS in going towards Lisbon from England, and in the Mediterranean, Dreadnought, Capt. GEDDES.

1730 May	Var ⁿ . P.M.	N. of O. C.	Lat.	Long ^{de} .	Var ⁿ . A.M.	N. of O. C.	Lat.	Long ^{de} .	
	5 13 26 W	2	45 22 N	9 39 W from London.					
	7 12 3	2	41 32	10 39					
1731 July 27	12 48	1							Island Sizarga S.E. 2 lgs. This island is about 5 lgs. to the W ^{ard} of the Giron.
	28 12 12	1	41 56	9 26					
	29 12 26	1	40 0	10 3					
8ber 8	11 10	1							The land about Cape Dragon in the Mediterranean N. by E. 12 or 14 lgs.
	11 10 52	1							Cape Toulon N.W. $\frac{1}{2}$ W. 6 lgs.
	20 9 42	2							In Leghorn Road.
8ber 6	10 48	1	41 34	18 54 W from London.					In our passage to England.

VARIATION of the COMPASS in the Mediterranean,
Hector, Sir ROGER BULTER.

1733 Apr.	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^{de} .	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^{de} .	
21	14 2W	1	34 2N	12 30E from London					Between the Island Pantalana and Tripoly in Barbary.
22	13.22	1							The observ ⁿ . not very good. In sight of the land to the East of Tripoly.
26	14 20	2	34 16	12 22					Between Tripoly and the Island Lampodoria.
7ber 17	13 38	3							In Gibraltar Bay.
27	13 49	3							Cape St. Vincent N. by E $\frac{1}{2}$ E. 4 lgs
9ber 9	13 56	2							Bound for Gibraltar towards Cape de Gatr.
11	14 12	2							Island of Alborne W. by S. 3 leagues.
18	14 34	1							Cape Mola on the Island Mi- norca N.N.E. 4 or 5 lgs.
Mar. 10	14 20	1							Cape St. Marys on the Coast of Portugal N.E. by E. 6 or 7 lgs.
1734 Apr. 6	13 56	3							On the Coast of Barbary near Larac.

1735 Aug.	Var ⁿ . P.M.	N ^o of Obs.	Lat.	Long ^d .	Var ⁿ . A.M.	N ^o of Obs.	Lat.	Long ^d .	
9	11 33W	2	38 8N	14 2W from London.					Going from the Coast of Barbary to Lisbon.
10	11 44	1	38 8	12 55					
1735 Aug.	12 19	3							In Sal'ee Road on the Coast of Barbary.

In Scandaroon Bay, 1711, I found 10° W. variation.

At Elfinore and Carlescroon in the Baltic, 1716, I found a point W^t. variation.

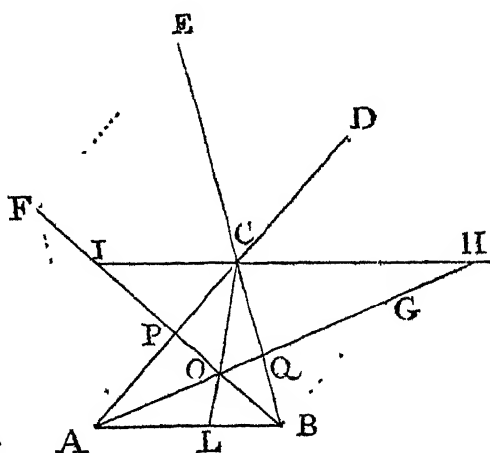
III. *Propositions selected from a Paper on the Division of Right Lines, Surfaces, and Solids. By James Glenie, A. M. of the University of Edinburgh. Communicated by the Astronomer Royal.*

R. June, 1775.

PROPOSITION I. THEOREM.

If from the angles at the base of any right-lined triangle, right lines be drawn to the alternate angles of rhombi, described upon the opposite sides, and applied reciprocally to the sides produced; and from the vertex, through the intersection of these lines, a right line be drawn to meet the base: the segments of the base, made thereby, will have to each other the duplicate proportion of the sides.

LET ACB be any right lined triangle. Let AFEC, CDGB be rhombi, on any two sides AC, CB of this triangle, applied respectively to CB, AC, produced: from the alternate angles EFA, DGB, of which let FA, GA, be right lines drawn to the angles at the base, or third side, AB. Then, if through the intersection O of



tion of AC to CB (11. E. 5.). But, since the triangles COH, COI, are respectively equiangular to the triangles AOL, LOB, the proportion of CH to IC is equal to the proportion of AL to LB (4. E. 6. and 16. E. 5.). Therefore the proportion of AL to LB is equal to the duplicate proportion of AC to CB (11. E. 5.). 2. *E. D.*

COR. I. If the triangle be isosceles, the right line drawn from the vertex to the base is perpendicular thereto, and the segments of the base are equal to each other.

COR. II. When the triangle is right-angled, the line drawn from the vertex to the base is always perpendicular to it (as appears from 8. E. 6. and its cor.); and the *rhombi* become squares on the sides comprehending the right angle.

COR. III. The segments of the sides adjacent to the base, are respectively third proportionals to the sum of the sides, and the sides themselves.

COR. IV. The segments of the sides adjacent to the vertex are equal to each other, and each of them is a fourth proportional to the sum of the sides, and the sides themselves (^a).

COR.

(a) And it may be added, a mean in proportion between the two segments adjacent to the base. For if a right line AB be any how divided in c, and from the two segments CA, CB, third proportionals to the whole line and each segment respectively, CD, CE, be taken away, the remainders AD, EB, are equal, and each is a mean in proportion between the two CD, CE. For because $AB : AC :: AC : CD$;

COR. v. The segments of the base are proportional to the segments of the sides, which are adjacent to them.

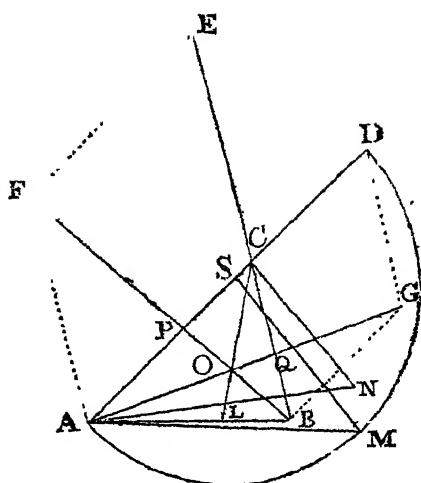
P R O P O S I T I O N I I .

Let there be any two right lines given. There is an angle which may be made by these lines; such that if, from their extremities which do not meet, right lines be drawn to the alternate angles of rhombi described on them, and reciprocally applied to them when produced; and from the said angle through the intersection of these lines, a right line be drawn to meet the right line joining the said extremities; the segments of this line made thereby, shall be respectively equal to the adjacent segments of the given lines.

therefore, by conversion, $AB : BC = CA : AD$. Again, because $AB : BC = BC : CE$, by conversion $AB : AC = BC : BE$: and by permutation $AB : BC = AC : BE$. Therefore $AC : BE = AC : AD$. Therefore AD and BE are equal. I say, moreover, that each of the two equal lines AD , BE , is a mean in proportion between the two CD , CE . For because $BA : AC = AC : CD$, by division $BC : CA = AD : DC$. Again, because $BA : BC = BC : CE$, converting and dividing $BC : CA = CE : EB$. Therefore, $CE : EB = AD : DC$. Q. E. D. s. HORSLEY.

LET

LET AC , CB , be any two given right lines, and let CD in AC produced be equal to CB . On AD describe a femi-circle; draw CN at right angles to AD , and equal to CD (11. E. 1.); join A , N , and apply a right line AM in the femi-circle equal to AN (1. E. 4.). From the point M draw the right line MS at right angles to AD (1. E. 4.)

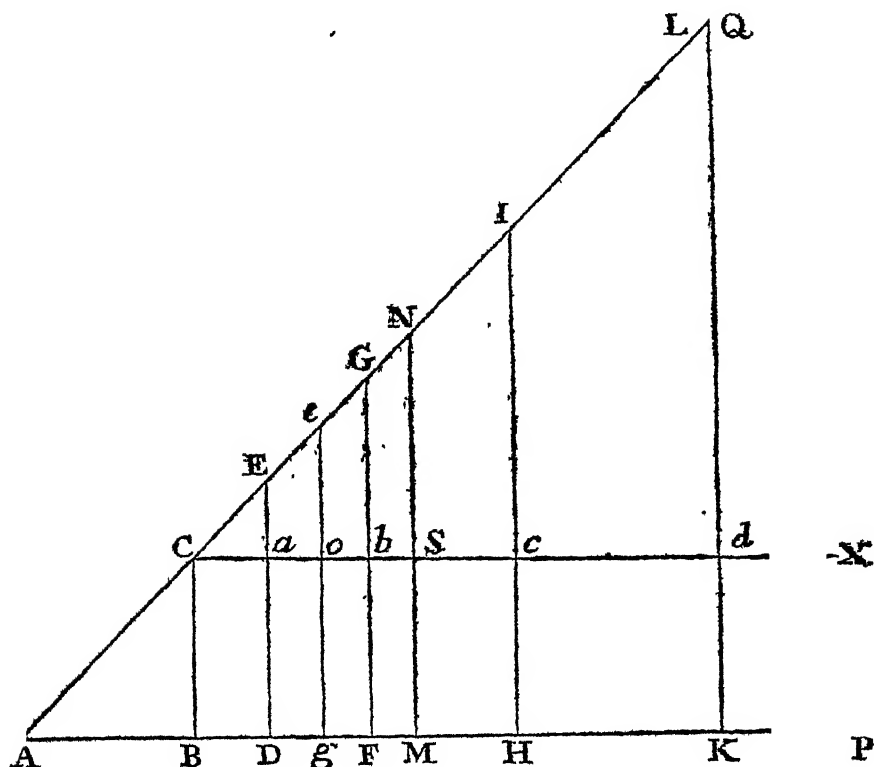


Make a triangle ACB , having its sides equal to AC , AS , and CB (by 22. E. 1.); and $\angle ACB$ is the angle required to be found; and the segments AL , LB , of the right line AB joining the extremities A and B of the given lines are respectively equal to the segments AP , BQ , of the given lines, which are adjacent to them. For the square on BC hath to the square on AC the duplicate proportion of BC to AC (cor. 1. to 20. E. 6.); that is, the proportion of BL to LA (prop. 1.). Wherefore the squares on AC , CB ; that is, the square on AN or AM (47. E. 1.) hath to the square on AC the proportion of AB to AL (18. E. 5.). But the square on AN or AM is equal to the rectangle contained by AD , AS (8. and 17. E. 6.). Wherefore the rectangle contained by AD , AS , hath to the square on AC , the proportion of AB to AL ; that is, the proportion of the rectangle contained by AB , AS to the rectangle contained by AL , AS (1. E. 6.). Consequently, the proportion of the rectangle AD , AS , to the rectangle

rectangle AB, AS; that is, the proportion of AD to AB (1. E. 6.) is equal to the proportion of the square on AC to the rectangle AL, AS (16. E. 5.) But AD hath to AC the proportion of AC to AP (cor. 3. to prop. 1.) Therefore, the rectangle AD, AC, hath to the square on AC the proportion of the rectangle AC, AS, to the rectangle AP, AS (1. E. 6.). Therefore AD hath to AS or AB the proportion of the square on AC to the rectangle AP, AS (16. E. 5. and 1. E. 6.). Hence the rectangle AL, AS, is equal to the rectangle AP, AS (11. and 16. E. 5.), and AL, AP, are consequently equal. But as AL to AP, so is BL to BQ (by cor. 5. prop. 1.). Therefore BL, BQ, are likewise equal. **Q. E. D.**

PROPOSITION III. PROBLEM I.

To multiply the square of a given finite right line by any number.



UPON an indefinite right line AP set off the given right line AB; draw BC at right angles to AP and equal to AB; and from A through c draw an indefinite right line AQ. Take AD equal to AC, and draw DE parallel to BC; AF equal to AE; and draw FG parallel to BC, and so on. Then it appears (from 47. E. I.), that the square of AC is

is equal to the square of AB multiplied by 2; the square of AE equal to the square of AD or AC multiplied by 2; that is, equal to the square of AB multiplied by 4, and so on. Thus the squares of AC , AE , AG , AI , AL , &c. are respectively equal to the square of AB multiplied by the terms of the following series 2, 4, 8, 16, 32, 64, &c. where the sixty-third term gives the square of AB multiplied by the last term of *SESSA'S* Series for the Chess-board.

If CX be drawn parallel to AP , the squares of Aa , Ab , Ac , Ad , &c. will be respectively equal to the square of AB multiplied by 3, 5, 9, 17, 33, 65, 129, &c. Also if Ag be taken equal to Aa , and ge be drawn parallel to BC , and this be repeated, the squares of Ae , &c. will be equal respectively to the square of AB multiplied by 6, 12, 24, 48, &c. And the squares on Ao , &c. will be equal to square on AB multiplied by 4, 7, 13, 25, 49, &c. In like manner, if AM be taken equal to Ab , and MN be drawn parallel to BC , the squares on AN , &c. will be equal respectively to the square on AB multiplied by 10, 20, 40, 80, 160, &c. And the squares on As , &c. will be equal respectively to the square on AB multiplied by the terms of the following series: 6, 11, 21, 41, 81, 161, &c.

In the same way, if right lines be drawn from E , e , G , N , I , L , &c. there will arise numberless other series. And if BC be taken equal to AB multiplied by any number, surd, fractional, or mixed, there will be obtained a great variety of series, consisting respectively of terms, which
are

are furd, fractional, or mixed. And by dividing BC, DE, *ge*, FG, MN, HI, KL, in different ways, according to pleasure, we may apply the same method to fractional numbers, without altering the magnitude of BC. Thus, if BC be bisectioned, and a right line be drawn through the point of bisection parallel to AP, there will be found right lines, the squares on which are respectively equal to the square on AB multiplied by a great number of fractions, having four for their common denominator, and so on.

PROPOSITION IV. PROBLEM II.

To find a right line, the square on which shall be equal to the square on a given right line, divided by any number.

IF, using the figure of the immediately preceding problem, we suppose the given right line to be denoted by AL, the squares on AK, AH, AF, AD, AB, &c. will respectively be equal to the square on AL multiplied by $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, $\frac{1}{32}$, $\frac{1}{64}$, $\frac{1}{128}$, $\frac{1}{256}$, $\frac{1}{512}$, $\frac{1}{1024}$, &c. or divided by 2, 4, 8, 16, 32, 64, 128, 256, 512, 1024, &c.; and so on for other numbers, whole, furd, fractional, or mixed.

Thus, if the square on BC be supposed successively equal to the square on AB multiplied by the terms of the series 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, &c. the numbers of the several parts denoted by AS, will be 11, 13, 15, 17, 19, 21, 23, 25, 27, 29, 31, 33, 35, 37, 39, 41, &c. which series comprehends all odd numbers after 9, and might have begun from 3 had the other series begun from 1.

number $m-1$, intersect BH in some point c. From the vertex A of the triangle BAC draw AL as was directed in prop. 1. and draw LS parallel to CA. I say BL is such a part of BC as is expressed by the number m ; and that BS is the same part of AB. For it appears (from prop. 1.) that BL is to CL as 1 to $m-1$. Wherefore BL is the m^{th} part of BC. And BS is the same part of AB that BL is of BC (4. E. 6.)

Thus if the square on AC be successively denoted by the square on AB multiplied by 3, 5, 7, 9, 11, 13, 15, 17, 19, 21, 23, 25, &c. BS will be successively such a part of AB as is expressed by 4, 6, 8, 10, 12, 14, 16, 18, 20, 22, 24, 26, 28, &c.

PROPOSITION VII. THEOREM II.

If from the angles at the base of any right lined triangle, right lines be drawn to the alternate angles of rhombi described on the other two sides, and reciprocally applied to them produced, and through the intersection of these lines, a right line be drawn from the vertex to the base; the rectangle contained by the sines of the angles at the extremities of one of the sides, will be equal to the rectangle contained by the sines of the angles at the extremities of the other^(b); and the parallelepiped contained by the sines of the

(b) The author means, that $\sin. ACL \times \sin. CAL = \sin. BCL \times \sin. CBL$ (see fig. prop. 1.). For $\sin. ACL : \sin. L = AL : AC$, and $\sin. L : \sin. BCL = BC : BL$. Take N, a third in proportion to AL, AC. Then, because $AC^2 : BC^2 = AL : LN$, N will

the angles of one of those triangles, into which the original one is divided by the said line drawn from the vertex. will be equal to the parallelepiped contained by the sines of the angles of the other.

COR. The two triangles, adjacent to the segments of the base, have to each other the proportion of the two adjacent to the sides containing the vertical angle, or the proportion of the two into which the original triangle is divided; and any one of these pairs of triangles are as similar figures described on the sides, being as the segments of the base, which have to each other the duplicate proportion of the sides.

PROPOSITION VIII. THEOREM III.

If from the angles at the hypotenuse of any right angled right lined triangle, right lines be drawn to the alternate angles of squares described on the sides containing the right angle, and from the point where the right line drawn from the right angle, through their intersection, meets the hypotenuse, right lines be drawn to the points, where these lines meet the sides; the lines so drawn will

likewise be a third in proportion to LB, BC. Hence, $\sin. ACL : \sin. L = AC : N$, and $\sin. L : \sin. BCL = N : BC$. *Ex æquo perturbate* $\sin. ACL : \sin. BCL = AC : BC$. But $\sin. CBL : \sin. CAL = AC : BC$. Therefore, $\sin. ACL : \sin. BCL = CBL : \sin. CAL$, and $\sin. ACL \times \sin. CAL = \sin. BCL \times \sin. CBL$. Q. E. D. And hence, $\sin. L \times \sin. ACL \times \sin. CAL = \sin. L \times \sin. BCL \times \sin. CBL$, which is the second branch of the proposition. S. HORSLEY.

make

COR. I. The alternate triangles of those four, which have their vertices in the point, where the right line drawn from the right angle meets the hypotenuse, are similar, and have to each other the proportion of the segments of the hypotenuse, or the duplicate proportion of the sides containing the right angle.

COR. II. Either pair of the adjacent triangles lying on different sides of the right line drawn from the right angle, and having their vertices in the intersection of the right lines drawn from the angles at the hypotenuse, have to each other the proportion of the alternate triangles, having their vertices in the intersection of the first-mentioned line and the hypotenuse.

COR. III. The trapezium or quadrilateral figure formed by the segments of the sides adjacent to the right angle, and the right lines joining their extremities with the intersection of the hypotenuse, and the right line drawn from the right angle to meet it, is capable of being inscribed in a circle; and is divided at the intersection of right lines drawn from the angles at the hypotenuse to the alternate angles of squares, described on the sides containing the right angle, into triangles which are proportional to one another, and when taken two by two, as

Further, the angles, $\angle PLA$, $\angle CLQ$, are equal, each being half a right angle. And the angles $\angle PAL$, $\angle LCQ$, are equal (by 8. Elem. 6.). Therefore the angles $\angle APL$, $\angle CQL$, will be equal, and the triangles $\triangle APL$, $\triangle CQL$, similar, and the sides subtending the equal angles proportional. Therefore, $PL : LA = QL : LC$. By permutation, $LP : LQ = LA : LC$. But $LA : LC = AC : CB$ (by 8. Elem. 6.). Therefore, $LP : LQ = AC : CB$. Q. E. D. & HORSLEY.

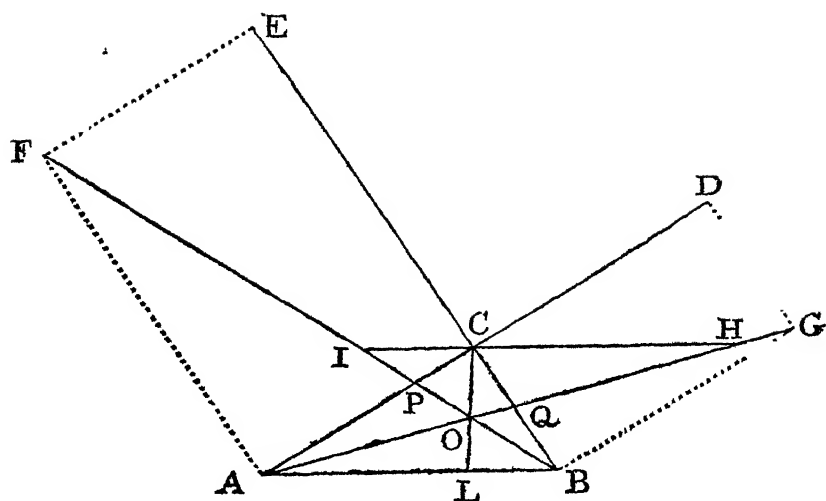
they

they lie adjacent on different sides of the diagonal, are proportional to the unequal sides of the trapezium, and to the two triangles into which the diagonal divides it. For $OPL : OPC = LO : OC = OQL : OQC$. Therefore, $OPL : OQL = OPC : OQC = LPC : LQC = LP : LQ$.

PROPOSITION IX. THEOREM IV.

If from the angles at the base of any right lined triangle, right lines be drawn to the alternate angles of rhomboids described on the other two sides, and reciprocally applied to them produced, a right line drawn from the vertex through the intersection of these lines will cut the base into two parts, having to each other the proportion compounded of the proportion of the sides, and of the proportion of the other two lines comprehending the rhomboids.

I SHALL subjoin a demonstration of this theorem, since the first proposition in these papers is only a particular case of it.



Let the triangle be ACB , the base AB , the rhomboids $ACFF$, $CCDG$; and let the right lines BF , AG , be drawn. Then, if from the vertex c through their intersection O , a right line COL be drawn to meet the base, the segments AL , LB , thereof will have to each other the proportion compounded of the proportions of AC to CB , and of CE to CD . For through the vertex c let a right line ICH be drawn parallel to AB , to meet BF , AG , produced, if necessary. Then, since the triangles CQH , CPI , are respectively equiangular to the triangles AQB , APB (15. and 29. E. 1.); the proportions of CH to AB , and of AB to IC are respectively equal to the proportions of CQ to QB , and of AP to PC (4. E. 6.). But the proportion of CH to IC is compounded of the proportions of CH to AB and of AB to IC ; and consequently is equal to a proportion compounded of the proportions of CQ to QB and of AP to PC . And since the triangles ACQ , APF , are respectively equiangular to

the triangles BQG, BPC (15. and 29. E. 1.); the proportions of CQ to QB, and of AP to PC, are respectively equal to the proportions of AC to BG or CD, and of AE or CE to CB. Wherefore the proportion of CH to IC is equal to the proportion compounded of the proportions of AC to CD and of CE to CB, or of AC to CB and of CE to CD. But since the triangles COH, COI, are respectively equiangular to the triangles AOL, LOB, the proportion of CH to IC is equal to the proportion of AL to LB (4. E. 6.). Therefore the proportion of AL to LB is equal to the proportion compounded of the proportions of AC to CB and of CE to CD. 2. E. D.

SCHOLIUM. If CE, CD, be equal to each other, AL hath to LB the proportion of AC to CB, and CL bisects the angle ACB; if CE have to CD the inverse proportion of AC to CB, AL is equal to LB; if CE have to CD the proportion of AC to CB, AL hath to LB the duplicate proportion of AC to CB; and universally, if CE have to CD any multiplycate proportion, n , of AC to CB, AL hath to LB such a multiplycate proportion of AC to CB as is expressed by the number $n+1$. And if CE have to CD any multiplycate proportion m of CB to AC, AL will have to LB such a multiplycate proportion of CB to AC, as is expressed by the number $m-1$.

IV. *A new Method of finding Time by equal Altitudes.*
 By Alexander Aubert, Esq. F. R. S.

R. Nov. 9,
 1775.

AMONG the various methods practised for finding time, that by equal altitudes of the Sun or of a star, hath hitherto been esteemed the most eligible for observers who are not furnished with a good and well-adjusted transit instrument. But this method, although one of the best, is generally attended with inconveniencies, which render the practice of it more difficult, and the result less perfect than one could wish.

If the Sun or stars near the equator are made use of, as usual, and the altitudes are taken near the prime vertical, where the change of altitude is the quickest, the interval of time between the observations must, in most latitudes, be of so many hours, that the observer cannot always attend to the corresponding altitudes: the weather may prove variable, so as to disappoint him at last; the clock or watch may go irregularly during so long an interval; and if the altitudes cannot, on account of their great distance from the meridian, be taken very high; an alteration in the state of the atmosphere may produce a variation of the refraction, and occasion the horary arcs to be different, although the apparent altitudes will be the same. To which may be added, the difficulty of
 making

making the instrument follow the object in its motion in azimuth, without danger of disturbing its adjustment in regard to altitude.

To remedy all these inconveniencies, the following method was thought of; and having been practised with constant success, it is presumed, the communication of it may be acceptable to astronomers.

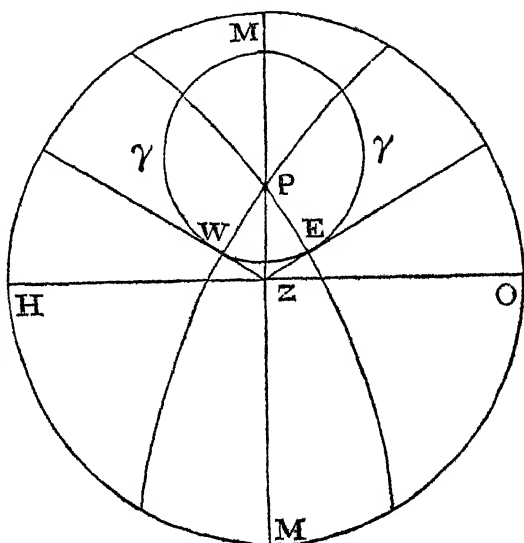
If a star is selected, of which the polar distance is very little less than the complement of the latitude of the place of observation, it will, at equal distances from the meridian, come to vertical circles, which touch its parallel of declination. The star, when in these vertical circles, will be near the meridian, near the prime vertical, and near the zenith; and consequently, if it be observed there, the interval between the Eastern and the Western altitudes will be short; the alteration in altitude will be quick; the star cannot be affected by a different refraction; besides, it will have no motion in azimuth.

To observe the star in these vertical circles, two things are necessary; the first is, to be provided with an astronomical quadrant, having three or more horizontal wires in the telescope, and if it have also a *speculum* at the eye-end of the telescope, to bring the vertical ray horizontal, it will be found very convenient. The next thing is, to make a computation of the apparent zenith distance of the star in the vertical circles which touch its parallel of declination; for if the telescope be fixed to this zenith distance, as soon as the star is
found

found to come to it, it will be in the proper vertical circle.

The true zenith distance is found easily in the following manner; and if diminished by the refraction belonging to it, it will give the apparent zenith distance wanted.

Let HMOM represent the horizon; Z the zenith; MM the meridian; P the pole of the equator; $\gamma\gamma$ the parallel of declination of a star, intersecting the meridian very near the zenith between Z and P. Then, the vertical



circles ZE on the Eastern side of the meridian, and ZW on the Western side, being drawn to touch the path of the star, the hour circles passing through the points of contact E, W, will be at right angles to the circles of azimuth ZE, ZW.

Now in the right-angled spheric triangle PEZ; PZ the complement of the latitude, and PE the star's polar distance, being known, the other parts are easily found; viz. ZE the zenith distance, PZE the azimuth, ZPE the horary angle. So that the telescope may be fixed properly, the position of the instrument is ascertained, and the time known when the star may be expected in the field

field of the telescope; and if the vertical wire is once brought near to the star, the star will appear to move parallel to it, and will pass successively over the horizontal wires, while the instrument remains undisturbed.

The advantage of this method will appear in the following example of equal altitudes, taken the 15th July, 1773, at Loam-pit Hill, near Deptford, in latitude $51^{\circ} 28' 7''$ N. and longitude $5''$ in time W. of the Royal Observatory at Greenwich.

The star selected was γ Draconis, having $38^{\circ} 28' 21''$ apparent North polar distance, being very little less than the complement of the latitude $38^{\circ} 31' 53''$.

Then $\cos. PE : \text{rad.} :: \cos. PZ : \cos. ZE = 2^{\circ} 19'$ the zenith distance,

and $\sin. PZ : \text{rad.} :: \sin. PE : \sin. PZE = 87^{\circ} 5' 20''$ the azimuth,

also $\text{rad.} : \tan. PE :: \cotan. PZ : \cos. ZPE = 3^{\circ} 43' 13''$ the horary arc = $14^{\circ} 52,9$

in sidereal time, or $14^{\circ} 50,5$ in mean time.

The true zenith distance being $2^{\circ} 19'$, the same was diminished by $2''$ for refraction, and the telescope fixed to $2^{\circ} 18' 58''$, the apparent zenith distance; and when the star came to the wires, the times by the clock were as follow:

	Eastern altitudes.			Western altitudes.			Meridian passage.		
	h	'	"		h	'	h	'	"
1st wire at	9	55	43	2' 14"	2' 14"	10	29	46	10 12 44,5
2d	9	57	57	2 12	2 12	10	27	32	10 12 44,5
3d	10	0	9			10	25	20	10 12 44,5

so that in about $34'$ the compleat set of altitudes was obtained near the prime vertical, free from the effects of a different refraction, and any motion in azimuth. The

horary

orary arc observed by the middle wire not turning out exactly according to the computation, is of no consequence to the observations. Some little difference may arise in it from small inaccuracies in the estimation of the star's apparent polar distance, the latitude of the place, or the error of the line of collimation; or from not setting the telescope exactly to the proper zenith distance; but as the chief intention of the computation is to find the vertical circles in which the star hath no motion in azimuth, the other parts of it need not be strictly attended to.

The following manner of inferring mean time from the star's meridian passage being more convenient and concise than the usual one, may also be acceptable.

From the star's apparent right ascension, increased by 24 hours if necessary, subtract the Sun's apparent right ascension for apparent noon; diminish the remainder by the proportional part of the star's acceleration, at the rate of $3' 55'',91$ for 24 hours, of which a table is easily computed; to this last remainder apply the equation of time for apparent noon, according as it is additive or subtractive; the result will be the mean time of the star's passing the meridian.

E X A M P L E.

The apparent \mathcal{R} of γ Draconis the 15th July, 1773, was	h ' "	17 51 24,0
— the apparent \mathcal{R} of the Sun at apparent noon,		7 39 59,0
First remainder,		10 11 25,0
— the star's acceleration for 10 11 25, at 3 55,91 for 24 hours		1 40,2
Second Remainder,		10 9 44,8
+ the equation of time at apparent noon, additive,		5 27,7
Star's meridian passage in mean time,		10 15 12,5

But the clock shewed 10^h 12' 44",5 when the star passed, consequently it was 2' 28",0 too slow for mean time.

Observers, who are not furnished with tables of the Sun's right ascension and of the equation of time for the apparent noon of their meridian, may apply both as they are given in the Nautical Ephemeris for the meridian of the Royal Observatory at Greenwich; the result will be the mean time of the star's passing the Greenwich meridian. And by applying the proportional part of the foregoing acceleration of 3' 55",91, belonging to the difference of longitude in time of the place of observation from Greenwich, the mean time of the star's passing the meridian of the place of observation will be found. If the place be to the East of Greenwich, the acceleration will be additive; if to the West, subtractive.

In a similar manner, the mean time of any observation made with a clock, regulated to fidereal time, may be inferred, provided the preceding transit of the Sun hath been observed; for if from the time of the observation by the clock, increased, if necessary, by 24 hours, the time of the observed transit of the Sun be subtracted, the remainder, diminished by the proportional part of $3' 55'', 91$ and duly corrected by the equation of time for the preceding noon, will give the mean time required. It is understood, that the clock keeps the rate of fidereal time exactly; for if not, a further correction for the loss or gain since noon must be applied.

V. *An Account of Falkland Islands.* By William Clayton, *Esq. of his Majesty's Navy.*

R. Nov. 9, 1775. **F**ALKLAND's Islands, or, as the Spaniards and French call them, the Maloine Islands, are situated between the latitude of $52^{\circ} 26'$ and $51^{\circ} 6' S.$ and longitude from London 56° to $60^{\circ} 30' W.$ They are numberless, forming a mass of broken high lands, or very low sedgey keys and funken rocks. The largest is the Easternmost island, and on the Eastern side the Spaniards had a settlement, which the crown of Spain purchased of M. BOUGAINVILLE, who, on his private account, had formed a settlement in the year 1764, at the time that Commodore BYRON had first discovered Port Egmont. The next large island is of a very considerable extent, and hath many excellent harbours on it. Between these two runs Falkland's Sound, which is navigable through; but the South entrance is pretty full of low sandy keys. Adjoining to the second large island, to the Westward, lies Saunders's Island, on which the English settlement was made, a blockhouse erected, several spots inclosed for gardens and three storehouses, and five dwelling-houses or huts, built at different times by the ships crews who were stationed there. The harbour of Port Egmont was formed by these islands, and

another high, barren, rocky island, named Kepple's Island, and some other lesser islands to the N.E. and Eastward, and was intirely land-locked, or inclosed by the land, on every point: it was very spacious; the bottom was muddy and good holding ground. From the hills through the bogs drained several runs of water, and as the landing-places were good, and a natural small cove for boats to lye in safety on the North-side of Saunders' Island sheltered from the S.W. winds, it induced Captain MACBRIDE to begin the settlement on it.

The larger islands are overspread with a short, tufty, round grass; a shrub with a smell like rosemary; a shrub of the myrtle kind, which in March and April bloffoms^(a); a white flower, of a faint violet smell; a small annual plant, of the wormwood kind. Near the shore, wherever there is a sandy soil, a species of grass grows, called Penguin grass, ~~from the birds of that species making their nests, and burrowing under ground like rabbits in holes.~~ This grass grows four or five feet high; the blades are broad and coarse, like rushes; the roots, when roasted, eat like almonds. Ground-forrel every where abounds in the greatest plenty; is extremely tart, and a most excellent antiscorbutic; the flower it produces is exactly like the wild rose which grows in the hedges in England. Celery, pepper-grass, and scurvy-grass, also abounds upon every island. Maiden hair (improperly so called) is plenty; the berries are ripe in February and March, and very plea-

(a) Lieutenant CLAYTON hath two or three of these shrubs in his garden at Peckham.

fant. A small species of cranberry abounds, and is the food of the wild geese all the autumn, when the geese are best. In the spring season, and part of the summer, there springs up an extreme pretty humble flower, which nearest resembles in leaf the auricula, but in flower the primrose; only they blow quite white. In very barren craggy spots, and even out of the cliffs of the rocks near the sea-shore, grows in the summer season, a small shrub which produces an uncommon but pretty flower, shaped like a lady's pocket; the colour is a rich yellow, I termed it queen's pocket: the seeds are very small. These are all the natural vegetable productions, and nothing rises to any size, nor doth any tree grow, on any of these islands^(b).

The prevailing winds are from the S. to the W. for two-thirds of the year, and in general boisterous and stormy. The N. and N.W. winds are mild and warm; but seldom of long continuance. The winds from the N.E. are moist, foggy, and unwholesome. From E. to S. are most pernicious, blighting, and tempestuous; they affect man, bird, beast, and vegetation: nothing can stand it which is exposed. Happily its duration is short; it seldom continues above 24 hours. It cuts the herbage down as if fires had been made under them; the leaves are parched up, and crumble to dust. The fowls are seized with cramps, so as to become lame, and never

(b) Wood strawberries grow on these islands, and are ripe in March; are of an earthy insipid taste, and grow to the size of the common small strawberry in England.

recover; but continue to decline till the whole side is decayed which was first affected. Hogs and pigs are suddenly taken with the staggers, turn round and drop, never to recover. Men are oppressed with a stopped perspiration, heaviness at the breast, fore throats; but they soon get over it, by due care.

The sea abounds with mullets, and some of a very large size up to ten pounds weight. Smelts in abundance, and as large as fourteen and fifteen inches in length; I have taken such with an angling line and rod. Transparent fish, shaped like a pike about the head, but not larger than a herring: these transparent fish are so clear when caught, that you may see through them; they have no red blood, but when cut a slimy water issues out, which I suppose is their blood. There are three or four species of the common loggerhead, or *sculpa* fish, common on the English coasts. A small sand-crab, small cray-fish, are to be got. Muscles are plenty, with limpets, and a few small clams. The muscles are very large and fine, and no way dangerous. In the river on the large island, are small fish like trout, very delicious; and no other sort whatever.

The amphibious animals are of four kinds, though seemingly of the same *genus*: the sea-lyon and the seal are distinct; the clapmatch seal and the fur seal are also distinct animals. The sea-lyon and lyoneses are bull-faced, with long shaggy hair; the common seal is smooth; the clapmatch is best pictured in Lord ANSON'S voyage, under the name of sea-lyon, in the drawings; the furr
seal

seal has its name from its coat, which is a fine soft furr, and is thinner skinned than any of the others. They all come on shore in December, to whelp their young; and remain mostly on land till they engender again. During this season it is rather dangerous coming near them, for the males are then vicious, and will endeavour to hurt any one who approaches their females; but at all other times they endeavour to make to the water, where they are safe. In mild warm days, during the summer, they come on shore, and lye basking in the Sun.

I consider the penguins as amphibious animals, partaking of the nature of birds, beasts, and fishes. There are four kinds; the yellow, or king penguin; the red; the black or holey, from their burrowing under ground; and the jumping jacks, from their motion. These creatures generally live in the sea, have very short wings which serve for fins, are covered with short thick feathers, and swim at an amazing rate. On shore they walk quite erect with a waddling motion, like a rickety child; and their breasts and bodies before being quite white, at a distance have, at first sight, the look of a child waddling along with a bib and apron on. They come on shore to lay and hatch their eggs in October: the yolks of the yellow, the holey, and jumping penguins, are yellow; but of the red penguins, it is red. All their eggs are good nourishing food, and a great refreshment to the seamen; but the flesh of these animals is coarse, fishy, and wholly unfit to eat.

The only beast on these islands is a fox, very nearly resembling the English fox; it is now very shy and scarce to be got.

Of birds. There are three sorts of wild geese: the mountain goose is somewhat larger than a Muscovy duck, feed always on the mountains, is pleasant tasted, and preferable to the other sorts, but is scarce. Its plumage on the back is speckled with brown and black, of a greenish hue, and towards the neck turns of a glossy beautiful golden colour; the breast is coloured like a pheasant. The other goose feeds in the vallies on the wild cranberries and grass, and is as large as a tame goose; the gander is black and white speckled; the goose is almost like the mountain goose, but darker and not so beautiful. These are good food in general; but best and fattest in February, March, and April. Of the sea-geese; the gander is white, the goose mottled, black and white; they feed always on the sea-shore, and are scarcely eatable. Wild ducks, widgeon, teal, and the shelldrakes, are the same as in Europe. But here is a species of ducks, called the loggerhead, from its large head. They have short wings, are unable to fly, and only swim and flap along on the water at an extraordinary rate. When driven ashore with boats they run fast, but soon squat down and are easily caught; they are eatable, but are but indifferent food: they are of a dark brown dirty colour. Snipes are plenty, and so exceedingly tame that we could shy at them with sticks, and get a dish whenever we wanted. Of small birds there are several sorts; the red breast, speckled

on the back like a partridge; the yellow breast; the white throat; the quaker, from its plumage being of the colour those people wear; the sparrow; tom-tit; linnets, and a bird like a goldfinch. Hawks are numerous; the eagle, the goshawk, the sparrow and the common hawk. Of every kind our crew ate, and found them very good and nourishing; owls there are not numerous.

Over the several islands is a surprizing species of vegetation, which I know not what to call. It resembles, at first view, our molehills in the marshy grounds in England. It is circular; sometimes six feet round, sometimes less. From the surface oozes out a gum in round blebs, of the smell and taste of balsam *capavia*. The body of these hills is formed within by a number of small substances, like the cones of pines. The outside is crusted over with dark green small leaves, running into each other, and cemented as if with glue. I opened several, and found that no vermin formed them; but there actually was a kind of vegetation; and yet the wild cranberries vegetate on them when the seed is lodged on them. The balsam I brought to England, and it is now on trial by an eminent surgeon.

Fern abounds, but is of a weaker sort than ours in Europe. We tried the furze-seed, and it came up; but so weak and poor that it would never increase or thrive. We found the season for sowing all kinds of garden seeds was about three weeks later than directed in the spring of ~~fall by~~ WILLER, remembering to reverse his months, calling September, March, &c.; but all kind of culinary

herbs and roots came to as great perfection as in England, and in great plenty, only we were forced to shelter every bed in the garden, by a good sod wall, from the S.W. and S.E. winds as much as possible, otherwise our labour would have been in vain.

The latter end of September or beginning of October, the sea birds begin to come on shore to build nests and lay. The first which appear are the albatrosses, which are about the size of a large goose, quite white, except their wings, which are a dark brown; the bills are of a dirty yellow, about three inches long; very strong and the edges sharp as a knife, hooked at the point; they breathe hard through two small holes in the bill close to the head, and frequently make a sound like a trumpet which children buy at fairs. Their wings are very long and narrow, with four joints in each wing, and extend ten or twelve feet from tip to tip. Their feet are webbed, very thin, have three claws; on the outer claw are four joints, the middle three, and the inner one. When they come to their towns, as we called their nesting-places, it is by hundreds. They set very tame, and continue one or other continually founding their bills. They never move off their nests let what will approach, and we shoved them off whenever we wanted their eggs. The egg is much larger than a goose's. The yolk is yellow; the white never boils hard, and always continues as clear asising-glass; our crew found them a good refreshment, though I thought them very strong. The nests of these birds are made on the ground with earth; are round, about

one foot high, and dented at top. While the hen sits, the male keeps constantly on the wing, and morning and evening returns with food to her. As soon as these birds have hatched, and the young ones are able to leave their nests, the jumping penguins repair to the nests and occupy them. The young albatrosses remain among them while the old ones go and seek food, with which they regularly return morning and evening. The season for every species of birds, wild and tame, laying and hatching is from September to December or January, and as all the eggs are very eatable, navigators touching at these islands in those months will meet great refreshment. In those three months we never meddled with the land geese, as they were breeding and could not be good.

The soil is in general boggy, barren, and rocky; but affords good pasturage in the vallies, and level spots for sheep and goats, and would for cattle, which might be out all winter; for that season is more remarkable for its mildness than in the same degree of Northern latitude. The summer is as remarkably cold, and both proceeds from the prevailing winds; in the winter the N. and N.E. winds are frequent, which brings warm, mild, moist weather. In the summer, the S. and S.W. and S.E. prevails, which are cold, sharp, and blighting; but in general, throughout the year, there is very little difference in the weather, but mostly cold. The thermometer scarce ever exceeds 64° in the warmest days, and very seldom in winter is below the freezing point, though I have seen it 20° below freezing; but that did not continue long,

nor does the snow continue in the plains or vallies a week together, or frost last so long; but the weather in winter is perpetually changing, the snow lies on the hills for nine months.

There is a great plenty, and some variety of moss on all the islands, and most of it when wet with water dyes of a brick-duft red. I tried it with other liquids, but found it still the same; so I believe it can be of no use.

The coasts of these islands abounds with whales of the spermaceti kind; the islands with innumerable seals and sea-lions, from whence a valuable fishery might, if thought proper, be carried on^(c). The passage out is twelve weeks; the same home. Ships might be loaded with oil ready made in six and eight weeks, and the price of that article greatly reduced.

These are all the remarks I made while I commanded on that barren, dreary, desolate, boggy, rocky spot, in

1774.

^(c) The year on which Lieutenant CLAYTON left the Falkland Islands, there were ten vessels from North America employed in whale fishing; and it is supposed, that the voyages answered very well, though in going out they commonly proceed as far to the Eastward as the Cape de Verde Islands.

VI. *Short and easy Theorems for finding, in all cases, the Differences between the Values of Annuities payable Yearly, and of the same Annuities payable Half-yearly, Quarterly, or Momently. By the Rev. Richard Price, D. D. F. R. S. In a Letter to Sir John Pringle, Bart. P. R. S.*

R. Nov. 9, 1775. **T**HE values of annuities, as given in all the common tables, suppose them paid yearly. But it is well known, that generally they are paid half-yearly, and sometimes quarterly; and that this is a circumstance which always adds to their value. The difference between the values of annuities, according as they are paid in these different ways, I have seen nowhere stated with accuracy; and therefore, I have thought that the following attempt to do this may be of some use.

Annuities are of two sorts. They are either payable certainly or conditionally. Of the former sort are all annuities which are payable at fixed times, without depending on any contingency. Of the latter sort are all annuities on lives. I will first consider the first sort of annuities.

Let r denote the interest of 1*l.* for a year; and n the term or number of years during which any annuity is to

be paid. Let P denote the value of the perpetuity, or the quotient arising from dividing 1 £. by its interest for a year. Let y denote the value of an annuity for n years, supposing it to be paid yearly; b its value, payable half-yearly; q its value, payable quarterly; and m its value, payable momentarily.

T H E O R E M I.

$$y = P - \frac{1}{r \times 1 + r^n}$$

T H E O R E M II.

$$b = P - \frac{1}{r \times 1 + \frac{r}{2}}^{2n}$$

T H E O R E M III.

$$q = P - \frac{1}{r \times 1 + \frac{r}{4}}^{4n}$$

T H E O R E M IV.

$m = P - \frac{1}{r^N}$ where N denotes the number which hath rn for its hyperbolic logarithm, and $rn \times 0.43429448$ for its logarithm in BRIGG's system.

E X A M P L E.

Let the rate of interest be 4 *per cent.* and the term 5 years, and consequently $r = 0.04$. $n = 5$, $P = 25$.

Then,

Then, $y=4.4518$

$b=4.4913$

$q=4.5120$

$m=4.5415$

EXAMPLE II.

Let the rate of interest be the same, and the term for which the annuity is payable 25 years.

Then, $y=15.6220$

$b=15.7118$

$q=15.7694$

$m=15.801$

EXAMPLE III.

Interest being the same, let the term be 50 years.

Then, $y=21.4822$

$b=21.5491$

$q=21.582$

$m=21.616$

EXAMPLE IV.

Interest being the same, let the term be 100 years.

Then, $y=24.505$

$b=24.523$

$q=24.532$

$m=24.542$

In the foregoing theorems it may be observed, that the *ratio* to one another, of the values of annuities payable yearly, half-yearly, quarterly; and momentarily, is greatest when n is least; that it decreases continually as n increases, till at last it vanishes, when n becomes infinite or the annuity is a perpetuity. Agreeably to this it appears; in the examples I have given, that the values in the first example differ more from one another in proportion than the values in the second example; and that these also differ more than the values in the third; and that in the last example all the values are nearly the same.

These values computed by Mr. DE MOIVRE's rules in his Treatise on Life-annuities, p. 86. and 124, &c. come out greater when n exceeds, and less when n falls short of 15 or 20 years. But those rules suppose the half-yearly and quarterly interests of money to be less than half or a quarter of the yearly interest. For instance; the value of an annuity of 1 £. payable half-yearly and quarterly for 50 years is, according to Mr. DE MOIVRE's rules, 21,699 and 21,772, or a 99th part and 74th part more than the value of the same annuity payable yearly, supposing money improved at 4 per cent. when the annuity is paid yearly; and at £. 1,98 per cent. when it is paid half-yearly; and at 0,985 per cent. when it is paid quarterly: That is; supposing money improved at a rate of half-yearly or quarterly interest, which, instead of being a half or a quarter of the yearly interest, is only that half-

half-yearly or quarterly payment which, in consequence of being laid up and improved at compound interest, will in a year amount to the sum that makes the yearly interest. It is obvious that this cannot be the proper method of computing these values. But not to insist on this; I will next state the different values of the second sort of annuities; or of life-annuities, according as they are supposed to be payable yearly, half-yearly, quarterly, or momentarily.

Let r as before be the interest of 1 £. for a year; n the complement of a given life^(a); y , b , q , and m , the values respectively of an annuity certain for n years payable yearly, half-yearly, quarterly, or momentarily; p the perpetuity; v the present value of an annuity on a life whose complement is n , payable yearly; h the value of the same annuity payable half-yearly; and q and m the values of the same annuity payable quarterly and momentarily.

(a) The complement of a life is, in Mr. DE MOIVRE's hypothesis, the number of years it wants of 86. In all other cases, it is double the expectation of a life; that is, it is double the quotient (diminished by $\frac{1}{2}$ unity) arising from dividing the sum of all the living in a table of observations from the age (inclusive) of the given life to the extremity of life, by the number of the living at that age. See Essay I. in my Treatise on Reversionary Payments.

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$$\text{Then, } Y = P - \frac{1+r}{nr} \times y.$$

$$H = P - \frac{1 + \frac{r}{2}}{nr} \times b.$$

$$Q = P - \frac{1 + \frac{r}{4}}{nr} \times q.$$

$$M = P - \frac{m}{nr}.$$

EXAMPLE I.

Let the life be supposed of the age of 36. The complement of such a life is 50, according to Mr. DE MOIVRE's hypothesis; and also very nearly, according to the Breslaw and the Northampton tables of observations. Therefore, n will be 50. Let the rate of interest be 4 *per cent*, or $r = 0,04$. $P = 25$. $y = 21,482$. $b = 21,549$. $q = 21,582$. $m = 21,616$. See p. 111.

$$\text{Therefore, } Y = 25 - \frac{1,04}{50 \times 0,04} \times 21,482 = 13,829.$$

$$H = 25 - \frac{1,02}{50 \times 0,04} \times 21,549 = 14,010$$

$$Q = 25 - \frac{1,01}{50 \times 0,04} \times 21,582 = 14,101$$

$$M = 25 - \frac{21,616}{50 \times 0,04} = 14,191$$

EXAMPLE II.

Let the life be supposed of the age of 61. The complement of this life is 25 by Mr. DE MOIVRE's hypothesis

his and the Northampton table of observations. Therefore, interest supposed at 4 *per cent*.

$$Y = 25 - \frac{1,04}{25 \times 0,04} \times 15,622 = 8,753$$

$$H = 25 - \frac{1,02}{25 \times 0,04} \times 15,712 = 8,973$$

$$Q = 25 - \frac{1,01}{25 \times 0,04} \times 15,769 = 9,072$$

$$M = 25 - \frac{15,801}{25 \times 0,04} = 9,199$$

The different values, given by these theorems, of life-annuities payable yearly, half-yearly, and quarterly, suppose nothing to be due to an annuitant for that year, half-year, or quarter, in which he shall happen to die. If, on the contrary, he is to be entitled to such part of the annuity as shall be proportioned to the time which shall happen to intervene between his death and the time when the payment immediately preceding his death became due; or in other words, if the annuity is an annuity secured by land, $\frac{y}{2n}$ must be added to the first theorem in order to obtain the value of such an annuity payable yearly. And in like manner, $\frac{b}{4n}$ must be added to the second theorem to obtain the value of the same annuity payable half-yearly: and $\frac{g}{8n}$ to the third theorem, to obtain its value payable quarterly.

The value, therefore, in the first example, of an annuity payable yearly on a life aged 36 being 13,829; its value, if secured by land, or to be enjoyed to the last moment of

life, will be $13,829 + \frac{21,482}{100} = 14,043$. If secured by land and payable half-yearly, its value will be $14,010 + \frac{21,549}{200} = 14,117$. If secured by land and payable quarterly, its value will be $14,101 + \frac{21,582}{400} = 14,155$. The like values in the second example are 9,065, 9,130, and 9,151.

Life-annuities payable monthly or weekly may be considered as of the same value with annuities payable momentarily; and it is evident, that they must be enjoyed nearly to the last moment of life.

From these rules and examples it may be gathered, that the difference between the values of annuities on lives payable yearly, half-yearly, quarterly, and momentarily, increases continually with the ages; but, if not secured by land, this difference can never be so great as a quarter of a year's purchase in the case of annuities payable yearly and half-yearly; three-eighths of a year's purchase in the case of annuities payable yearly and quarterly; and half a year's purchase in the case of annuities payable yearly and momentarily.

Mr. SIMPSON, in his *Treatise on the Doctrine of Life-annuities*, p. 78. and in his *Select Exercises*, p. 283. hath given a quarter of a year's purchase as the addition always to be made to the value of a life-annuity payable yearly, in order to obtain its value payable half-yearly; and three-eighths of a year's purchase, if its value payable quarterly is required. But it appears, that these are too large additions; and, whatever be the rate of interest,

or the number of lives, a fifth of a year's purchase will be generally more than a sufficient addition, if the value of the annuity is desired payable half-yearly; and three-tenths of a year's purchase, if the value of the annuity is desired payable quarterly. Mr. DE MOIVRE's rules, in p. 85 of his Book on Life-annuities, for finding the values of life-annuities payable half-yearly and quarterly from their values payable yearly, are still less correct; for they suppose the difference between these values the same, whether the annuities are life-annuities, or annuities certain.

Mr. DODSON, in the first question in the third volume of his Mathematical Repository, hath given a rule for finding the value of an annuity secured by land and payable yearly, which coincides with that here given; and Mr. DE MOIVRE, in p. 338. of his Treatise on the Doctrine of Chances, hath given a theorem for this purpose, which also brings out nearly the same answers. But Mr. SIMPSON, in prob. I. p. 323. of his Select Exercises, makes the excess of the value of such an annuity above the value of an annuity payable yearly, but not secured by land, double to the same excess derived from Mr. DODSON's and Mr. DE MOIVRE's rules. The truth is, that Mr. DODSON's rule gives the exact value; and that Mr. SIMPSON's problem gives the value, not of an annuity secured by land and payable yearly, but of an annuity secured by land and payable momentarily; and also, that his method of solution implies a rate of interest somewhat less when the annuity is payable momentarily than when it is payable yearly.

But

But to prevent all perplexity on this subject, I will subjoin the following investigations, which will be easily understood by those who are acquainted with the common methods of calculating the values of life-annuities.

Let r , as before, be the interest of £. 1 for a year. Then the present value of £. 1 payable at the end of one year, two years, three years, &c. will be $\frac{1}{1+r}$, $\frac{1}{1+r^2}$, $\frac{1}{1+r^3}$, &c. respectively. And the present value of an annuity certain for n years payable yearly is the sum of this series continued to n terms ^(b), or $\frac{1}{r} - \frac{1}{r \times \frac{1}{1+r}^n} = P - \frac{1}{r \times \frac{1}{1+r}^n} = y$.

In like manner, the present value of half £. 1 (that is of 10 s. = £. 0,5) payable at the end of half a year, a year, a year and a half, &c. reckoning half-yearly interest at half the annual interest, is $\frac{0,5}{1+\frac{r}{2}}$, $\frac{0,5}{1+\frac{r}{2}^2}$, $\frac{0,5}{1+\frac{r}{2}^3}$, &c. And the pre-

sent value of an annuity certain payable half-yearly for n years, each payment to be half the yearly payment, is the sum of this series continued to $2n$ terms; or,

$$\frac{0,5}{\frac{r}{2}} - \frac{0,5}{\frac{r}{2} \times 1 + \frac{r}{2}} = \frac{1}{r} - \frac{1}{r \times 1 + \frac{r}{2}} = P - \frac{1}{r \times 1 + \frac{r}{2}} = b.$$

(b) In the postscript it will be proved, that the sum of n terms of the series $\frac{1}{a} + \frac{1}{a^2} + \frac{1}{a^3} + \frac{1}{a^4}$, &c. is $\frac{1}{a-1} - \frac{1}{a^n \times a-1}$. Substitute $1+r$ for a , and it will appear, that the sum of n terms of the series $\frac{1}{1+r} + \frac{1}{1+r^2} + \frac{1}{1+r^3}$, &c. is

$$\frac{1}{r} - \frac{1}{r \times \frac{1}{1+r}^n}.$$

By the same steps it will appear, that the present value of an annuity certain for n years to be received in quarterly payments, each a quarter of the annual payment, is

$$\frac{0,25}{\frac{1}{4}r} - \frac{0,25}{\frac{1}{4}r \times 1 + \frac{r}{4}} \sqrt[4]{r^{4n}} = P - \frac{1}{r \times 1 + \frac{r}{4}} \sqrt[4]{r^{4n}} = q. \quad \text{And also, that the pre-}$$

sent value of an annuity certain for n years, to be received in momentarily payments, each the same proportional part of the yearly payment that the moment is of

the year, must be $P - \frac{1}{r \times 1 + \frac{r}{1000, \&c. n}} \sqrt[1000, \&c. n]{r^{1000, \&c. n}}$. But, by the

binomial theorem, $1 + \frac{r}{1000, \&c. n} \sqrt[1000, \&c. n]{r^{1000, \&c. n}} = 1 + rn + \frac{r^2 n^2}{2} + \frac{r^3 n^3}{2 \times 3} + \frac{r^4 n^4}{2 \times 3 \times 4}, \&c.$ which series approximates indefinitely to

that number of which rn is the hyperbolic logarithm, by prob. 1. sect. XI. vol. II. of Mr. SIMPSON's Fluxions; or by prop. I. p. 40. of his Treatise on Trigonometry. Therefore,

$$P - \frac{1}{r \times 1 + \frac{r}{1000, \&c. n}} \sqrt[1000, \&c. n]{r^{1000, \&c. n}} = P - \frac{1}{rN} = M, \text{ as explained before.}$$

See p. 110.

If the value of an annuity of £. 1 for n years is required payable half-yearly, and the half-yearly interest of £. 1, instead of being half the yearly interest

(or $\frac{r}{2}$), is supposed to be $\sqrt[1]{1+r}^{\frac{1}{2}} - 1$; the answer will be

$$\frac{0,5}{1+r^{\frac{1}{2}}} + \frac{0,5}{1+r} + \frac{0,5}{1+r^{\frac{3}{2}}} + \frac{0,5}{1+r^2}, \&c. \text{ continued to } 2n \text{ terms} =$$

$$= \frac{0,5}{\overline{1+r}^{\frac{1}{2}} - 1} - \frac{0,5}{\overline{1+r}^n \times \overline{1+r}^{\frac{1}{2}} - 1} = 1 - \frac{1}{\overline{1+r}^n} \times \frac{1}{2 \times \overline{1+r}^{\frac{1}{2}} - 2}; \text{ which va-}$$

lue is to, $1 - \frac{1}{\overline{1+r}^n} \times \frac{1}{r}$ (the value of the same annuity payable yearly supposing the yearly interest of £. 1 to be r) as $\frac{\frac{1}{2}}{\overline{1+r}^{\frac{1}{2}} - 1}$ to $\frac{1}{r}$, agreeably to Mr. DE MOIVRE's deduction

in his Treatise on Life-annuities, p. 125. fourth edit. This implying, in the case of annuities payable half-yearly, a smaller interest than half the yearly interest (for $\overline{1+r}^{\frac{1}{2}} - 1$ is less than $\frac{r}{2}$) gives the difference between their value and the value of annuities payable yearly, greater than the truth.

But to return to the investigation of the theorems in the former part this paper.

Let us again call p the perpetuity, and y the value of an annuity certain for n years and payable yearly; it is well known that the value of £. 1 payable yearly on a life whose complement is n is (supposing an equal decrement of life) $\frac{n-1}{n \times \overline{1+r}} + \frac{n-2}{n \times \overline{1+r}^2} + \frac{n-3}{n \times \overline{1+r}^3}$, &c. continued to n terms (c) $= p - \frac{1+r}{nr} \times y = Y$.

In

(c) See Mr. DE MOIVRE's Treatise on Life-annuities, p. 99. fourth edit. Or his Doctrine of Chances, p. 311. third edition. Or Mr. DODSON's Mathematical Repository, vol. II. p. 137. Or Mr. SIMPSON on Annuities and Reversions, p. 14. In consulting these writers, care should be taken to remember, that they use r to denote the principal and interest of £. 1 for a year; whereas it hath

In like manner, supposing money improved at an half-yearly interest equal to half the yearly interest, or to $\frac{r}{2}$, the value of the same annuity payable half-

been most convenient for me in these observations to make r stand only for the interest. In these writers, therefore, r signifies the same with $1+r$ in this paper; and $r-1$ the same with r .

It is said above, that the value of an annuity payable yearly, on a life whose complement is n , is $\frac{n-1}{n \times 1+r} + \frac{n-2}{n \times 1+r^2} + \frac{n-3}{n \times 1+r^3}$, &c. continued to n terms.

This expression is equal to $\frac{n}{n \times 1+r} + \frac{n}{n \times 1+r^2} + \frac{n}{n \times 1+r^3}$, &c. (n)
 $= \frac{1}{n} \times \frac{1}{1+r} + \frac{2}{1+r^2} + \frac{3}{1+r^3}$, &c. (n). But $\frac{n}{n \times 1+r} + \frac{n}{n \times 1+r^2} + \frac{n}{n \times 1+r^3}$

&c. ($= \frac{1}{1+r} + \frac{1}{1+r^2} + \frac{1}{1+r^3}$, &c.) $= \frac{1}{r} - \frac{1}{r \times 1+r^n} = y$ (see p. 118.) Also, by a theorem which will be demonstrated in the postscript, and putting a for any given quantity, $\frac{1}{a} + \frac{2}{a^2} + \frac{3}{a^3}$, &c. continued to n terms =

$\frac{a}{a-1} - \frac{n}{a^n} \times \frac{1}{a-1} - \frac{1}{a^n} \times \frac{a}{a-1}$. Therefore, if $1+r$ is substituted for a ,

and y for $\frac{1}{r} - \frac{1}{r \times 1+r^n}$, the sum (multiplied by $\frac{1}{n}$) of n terms of the series

$\frac{1}{1+r} + \frac{2}{1+r^2} + \frac{3}{1+r^3}$, &c. will come out $\frac{1+r}{nr} \times y - \frac{1}{r} \times \frac{1}{1+r^n}$; or

$\frac{1+r}{nr} \times y + y - \frac{1}{r}$. Therefore, the series $\frac{1}{n} \times \frac{1}{1+r} + \frac{2}{1+r^2} + \frac{3}{1+r^3}$, &c. con-

tinued to n terms and subtracted from the series $\frac{1}{1+r} + \frac{1}{1+r^2} + \frac{1}{1+r^3}$, &c. con-

tinued to n terms; that is, the value of the life will be $y - \frac{1+r}{nr} \times y + y - \frac{1}{r} =$

$\frac{1}{r} - \frac{1+r}{nr} \times y = p - \frac{1+r}{nr} \times y = x$.

yearly, is $\frac{1}{2} \times \frac{n-\frac{1}{2}}{n \times 1 + \frac{r}{2}} + \frac{n-1}{n \times 1 + \frac{r}{2}} + \frac{n-\frac{3}{2}}{n \times 1 + \frac{r}{2}}$, &c. continued to

$2n$ terms $= \frac{1}{2} \times \frac{n}{n \times 1 + \frac{r}{2}} + \frac{n}{n \times 1 + \frac{r}{2}} + \frac{n}{n \times 1 + \frac{r}{2}}$, &c. continued to

$2n$ terms $= \frac{1}{2} \times \frac{\frac{1}{2}}{n \times 1 + \frac{r}{2}} + \frac{1}{n \times 1 + \frac{r}{2}} + \frac{\frac{3}{2}}{n \times 1 + \frac{r}{2}}$, &c. continued

to $2n$ terms. But the sum of the first of these two series, or of $\frac{1}{2} \times \frac{n}{n \times 1 + \frac{r}{2}} + \frac{n}{n \times 1 + \frac{r}{2}}$, &c. $(= \frac{1}{2} \times \frac{1}{1 + \frac{r}{2}} + \frac{1}{1 + \frac{r}{2}})$, &c.) is b ,

see p. 118. And the sum of the second series is the same

with half the sum of the series $\frac{1}{2n} \times \frac{1}{1 + \frac{r}{2}} + \frac{2}{1 + \frac{r}{2}} + \frac{3}{1 + \frac{r}{2}}$, &c.

($2n$). But by the theorem mentioned in the last

note, the sum of n terms of the series $\frac{1}{a} + \frac{2}{a} + \frac{3}{a}$, &c. is

$\frac{a}{a-1} - \frac{n}{a^n} \times \frac{1}{a-1} - \frac{1}{a^n} \times \frac{a}{a-1}$. Therefore, if $1 + \frac{r}{2}$ is substituted for a , $2n$ for n , and b for $\frac{1}{r} - \frac{1}{r \times 1 + \frac{r}{2}}$, the sum of

the second series (that is, of $\frac{1}{2} \times \frac{1}{2n} \times \frac{1}{1 + \frac{r}{2}} + \frac{2}{1 + \frac{r}{2}} + \frac{3}{1 + \frac{r}{2}}$, &c.

($2n$) will come out $\frac{1 + \frac{r}{2}}{nr} \times b - \frac{1}{r} \times \frac{1}{1 + \frac{r}{2}}$, or $\frac{1 + \frac{r}{2}}{nr} \times b + b - \frac{1}{r}$.

Therefore,

Therefore, the second series subtracted from the first,

leaves $\frac{1}{r} - \frac{1 + \frac{r}{2}}{nr} \times b = P - \frac{1 + \frac{r}{2}}{nr} \times b = H$, agreeably to the second theorem in p. 114.

By reasoning in the same way it may be easily found,

that $Q = P - \frac{1 + \frac{r}{4}}{nr} \times q$; and $M = P - \frac{1 + \frac{r}{1000, \&c.}}{nr} \times m = P - \frac{m}{nr}$, agreeably to the third and fourth theorems in p. 114.

These theorems, I have said, suppose that an annuitant is entitled to no payment for that year, half-year, or quarter, in which he dies. If, on the contrary, he is to be intitled, when he dies, to such a part of the yearly, half-yearly, or quarterly payment as shall bear the same proportion to the said payments respectively, as the intermediate time between the last payment and his death bears to the whole year, half-year, or quarter; in this case, supposing the annuity payable yearly, it is evident, since there is the same chance for his dying in one half of any year as in the other, that he will have an expectation of half a year's payment more than he would be otherwise intitled to. But the value of half £. 1 to be paid at the death of a person whose complement of life is n , is $\frac{1}{2} \times \frac{1}{n \times 1 + r} + \frac{1}{2} \times \frac{1}{n \times 1 + r}^2 + \frac{1}{2} \times \frac{1}{n \times 1 + r}^3$, &c. continued to n terms $(d) = \frac{y}{2n}$.

(d) P. 118.

In like manner, a person who enjoys an annuity secured by land, payable half-yearly, will have an expectation of a quarter of a year's payment more than he could be otherwise intitled to; the value of which is

$$\frac{1}{4^n} \times \frac{1}{1 + \frac{r}{2}} + \frac{1}{1 + \frac{r}{2}} + \frac{1}{1 + \frac{r}{2}} + \dots \text{continued to } 2n \text{ terms} = \frac{b}{4^n}.$$

By the same reasoning it will appear, that $\frac{q}{8^n}$ is the addition to be made to the value of an annuity payable quarterly, in order to obtain its value when secured by land.

P O S T S C R I P T.

IN the note p. 121. the expression $\frac{1}{a-1} - \frac{1}{a^n} \times \frac{1}{a-1}$ is given as the sum of n terms of the series $\frac{1}{a} + \frac{1}{a^2} + \frac{1}{a^3} + \frac{1}{a^4}$, &c. to $\frac{1}{a^n}$, and the expression $\frac{a}{a-1} - \frac{n}{a^n} \times \frac{1}{a-1} - \frac{1}{a^n} \times \frac{q}{a-1}$, is given as the sum of n terms of the series $\frac{1}{a} + \frac{2}{a^2} + \frac{3}{a^3} + \frac{4}{a^4}$, &c.

The following investigation of these theorems being very easy, will not, perhaps, be unacceptable to those who have studied this subject.

$$\text{Put } A = \frac{1}{a} + \frac{1}{a^2} + \frac{1}{a^3} + \frac{1}{a^4}, \text{ \&c. } \frac{1}{a^n}. \quad B = \frac{1}{a} + \frac{2}{a^2} + \frac{3}{a^3} + \frac{4}{a^4}, \text{ \&c. } \frac{n}{a^n}.$$

$$\text{Then } A \times a = 1 + \frac{1}{a} + \frac{1}{a^2} + \frac{1}{a^3}, \text{ \&c. to } \frac{1}{a^{n-1}},$$

$$\text{and } A \times a - 1 + \frac{1}{a^n} = \frac{1}{a} + \frac{1}{a^2} + \frac{1}{a^3}, \text{ \&c. to } \frac{1}{a^{n-1}} + \frac{1}{a^n} = A,$$

and

$$\text{and } A \times a - A (= A \times \overline{a-1}) = 1 - \frac{1}{a^n}.$$

Therefore, $A = \frac{1}{a-1} - \frac{1}{a^n} \times \frac{1}{a-1}$, which is the first theorem.

$$\text{Again, } A \times a = 1 + \frac{1}{a} + \frac{1}{a^2} + \frac{1}{a^3}, \text{ \&c. to } \frac{1}{a^{n-1}},$$

$$\text{and } B \times a = 1 + \frac{2}{a} + \frac{3}{a^2} + \frac{4}{a^3}, \text{ \&c. to } \frac{n}{a^{n-1}}.$$

$$\text{Therefore, } B \times a - A \times a = \frac{1}{a} + \frac{2}{a^2} + \frac{3}{a^3}, \text{ \&c. to } \frac{n-1}{a^{n-1}}.$$

To both sides of the last equation add $\frac{n}{a^n}$, and it will appear, that

$$B \times a - A \times a + \frac{n}{a^n} = \frac{1}{a} + \frac{2}{a^2} + \frac{3}{a^3} + \frac{4}{a^4} +, \text{ \&c. to } \frac{n-1}{a^{n-1}} + \frac{n}{a^n} = B.$$

$$\text{Therefore, } B \times a - B = B \times \overline{a-1} = A \times a - \frac{n}{a^n}; \text{ and } B = \frac{A \times a}{a-1} - \frac{n}{a^{n+1}-a^n}.$$

For A, in this last equation, substitute its equal, or $\frac{1}{a-1} - \frac{1}{a^n} \times \frac{1}{a-1}$, and the resulting equation will be $\frac{a}{a-1} - \frac{n}{a^n} \times \frac{1}{a-1} - \frac{1}{a^n} \times \frac{a}{a-1} = B$, which is the second theorem.

When n is infinite, all but the first terms in both these theorems vanish; and therefore, $\frac{1}{a-1}$ is the sum of the series $\frac{1}{a} + \frac{1}{a^2} + \frac{1}{a^3}, \text{ \&c. continued infinitely; and } \frac{a}{a-1}$ is the sum of the series $\frac{1}{a} + \frac{2}{a^2} + \frac{3}{a^3}, \text{ \&c. continued infinitely.}$

By a like deduction, putting

$$C = \frac{1}{a} + \frac{2 \times 2}{a^2} + \frac{3 \times 3}{a^3} + \frac{4 \times 4}{a^4}, \text{ \&c. to } \frac{n^2}{a^n},$$

and $D = \frac{1}{a} + \frac{2 \times 2 \times 2}{a^3} + \frac{3 \times 3 \times 3}{a^3} + \frac{4 \times 4 \times 4}{a^4}, \text{ \&c. to } \frac{n^3}{a^n}$, it may be found that $C = \frac{A+2B+1}{a-1} - \frac{(n+1)^2}{a^{n+1}-a^n}$, and $D = \frac{A+3B+3C+1}{a-1} - \frac{(n+1)^3}{a^{n+1}-a^n}$.

And consequently, substituting the values of A and B, that

$$C = \frac{a^2+a}{(a-1)^3} - \frac{n^2}{a^n} \times \frac{1}{a-1} - \frac{2an}{a^n} \times \frac{1}{(a-1)^2} - \frac{a^2+a}{a^n} \times \frac{1}{(a-1)^3}.$$

And, substituting the values of A, B, C, that

$$D = \frac{a^3+4a^2+a}{(a-1)^4} - \frac{n^3}{a^n} \times \frac{1}{a-1} - \frac{3an^2}{a^n} \times \frac{1}{(a-1)^2} - \frac{3a^2n+3an}{a^n} \times \frac{1}{(a-1)^3} - \frac{a^3+4a^2+a}{a^n} \times \frac{1}{(a-1)^4}.$$

Or, since all but the first terms in these expressions vanish when n is infinite, that the sum of the series $\frac{1}{a} + \frac{4}{a^2} + \frac{9}{a^3}, \text{ \&c. continued infinitely is } \frac{a^2+a}{(a-1)^3}$; and that the sum of the series $\frac{1}{a} + \frac{8}{a^2} + \frac{27}{a^3} + \frac{64}{a^4}, \text{ \&c. continued infinitely is } \frac{a^3+4a^2+a}{(a-1)^4}.$

These are all the theorems necessary for calculating the values of annuities on single lives, and on any two or three joint lives, upon the hypothesis of an equal decrement of life.

Supposing r the interest of £. 1 for a year, the sum of n terms of the series $\frac{1}{1+r} + \frac{1}{1+r^2} + \frac{1}{1+r^3}, \text{ \&c.}$ is the present value of an annuity certain for n years; and

$$\frac{1}{1+r}$$

$\frac{1}{1+r} + \frac{2}{(1+r)^2} + \frac{3}{(1+r)^3} + \frac{4}{(1+r)^4}$, (continued to n terms) is the present value of an annuity certain beginning with £. 1, and increasing to £. 2 the second year, to £. 3 the third year, &c.

If this last annuity is not an annuity certain for a given term, but a life-annuity, the value of it (supposing n the complement of life, A the value of an annuity certain for n years, G the value of two equal joint lives whose common complement is n , P the perpetuity, and p the value of £. 1 to be received at the end of n years) will be $A - G \times n + n \times p \times P - A \times P \times \frac{1}{1+r}$.

EXAMPLES.

Let the term be forty-one years, and the rate of interest 4 per cent.

The value of an annuity of £. 1 certain for this term is £. 20.

The value of an annuity certain for the same term, and beginning with £. 1 at the end of the first year, but increasing to £. 2 at the end of the second year, to £. 3 at the end of the third year, and so on till it becomes £. 41 at the end of the forty-first year, is (by the second theorem, putting $1+r$, or 1.04 for a) £. 214 10s.

The value of an annuity increasing at this rate without end is £. 650.

If

If the annuity is a life-annuity which is to increase at the rate of £. 1 every year during the whole possible continuance of a life whose complement is forty-one years (or whose age is about forty-five) the present value of it will be, by the last theorem, £. 133 14s.; taking the probability of the duration of human life according to Mr. DE MOIVRE's hypothesis; which agrees nearly with Dr. HALLEY's Table of Observations.

VII. *An Account of the Romanſh Language.* By Joſeph Planta, F. R. S. *In a Letter to Sir John Pringle, Bart. P. R. S.*

S I R,

British Muſeum,
June 10, 1775.

R. Nov. 10, 1775. **T**HE bible lately preſented to the Royal Society by the Count DE SALIS, being a verſion into a language as little attended to in this country, as it may appear curious to thoſe who take pleaſure in philological inquiries; I embrace this opportunity to communicate to you, and, with your approbation, to the Society, all that I have been able to collect concerning its hiſtory and preſent ſtate.

This language is called *Romanſh*, and is now ſpoken in the moſt mountainous parts of the country of the Grifons, near the ſources of the Rhine and the En. It conſiſts of two main dialects; which, though partaking both of the above general name, differ however ſo widely as to conſtitute in a manner two diſtinct languages. Books are printed in both of them; and each, though it be univerſally underſtood in its reſpective diſtrict, is yet ſub-divided into almoſt as many ſecondary dialects as there are villages in which it is ſpoken; which differ, however, but little except in the pronunciation. One of the main dialects, which is ſpoken in the Engadine, a valley extending from

the source of the En to the frontiers of the Tyrolese, is by the inhabitants called *Ladin*. It admits of some variation, even in the books, according as they are printed either in the upper or the lower part of this province. The abovementioned bible is in the dialect of the lower Engadine; which, however, is perfectly understood in the upper part of that province, where they use no other version. The other dialect, which is the language of the Grey, or Upper League, is distinguished from the former by the name of *Cialover*: and I must here observe, that in the very center, and most inaccessible parts of this latter district, there are some villages situated in narrow vallies, called Rheinwald, Cepina^(a), &c. in which a third language is spoken, more similar to the German than to either of the above idioms, although they be neither contiguous, nor have any great intercourse with the parts where the German is used.

It being impossible to form any idea of the origin and progress of a language, without attending to the revolutions that may have contributed to its formation and subsequent variations; and this being particularly the case in the present instance, wherein no series of documents is extant to guide us in our researches; I shall briefly recapitulate the principal events which may have affected the language of the Grisons, as I find them related by authors of approved veracity^(b).

AMBI-

(a) TSCHUDI *Rhet. Descrip.* p. 43: MERIAN *Topogr. Helvet.* p. 64.

(b) SPRECHER, SIMLER, TSCHUDI, SCHEUCHZER. 'CAMPELL'S Chronicle is looked upon as the most authentic and circumstantial; but there being only
a few

AMBIGATUS, the first king of the Celtic Gaul upon record, who (c) about 400 (d) years before CHRIST, governed all the country situated between the Alps and the Pyrenæan mountains, sent out two formidable armies under the command of two of his nephews; one of whom, named SEGOVISIUS, forced his way into the heart of Germany: and the other, BELLOVISIUS, having passed the Alps, penetrated into Italy as far as the settlements of the Tuscans, which at that time extended over the greatest part of the country now called Lombardy. These, and several other swarms of invaders whom the successes of the former soon after attracted, having totally subdued that country, built Milan, Verona, Brescia, and several other considerable towns, and governed with such tyrannic sway, especially over the nobility, whose riches they coveted and sought by every means to extort from them, that most of the principal families, joining under the conduct of RHÆTUS (e), one of the most distinguished personages among them, retired with the best part of their effects and attendants among the steepest mountains of the Alps, near the sources of the Rhine, into the district which is now called the Grey League.

a few manuscript copies of it extant in the hands of private persons in the Grisons, I have not been able to avail myself of his researches. GULER and STUMPFUS might also have furnished some material information; but neither of them have I had an opportunity of inspecting.

(c) LIV. lib. v. c. 34.

(d) Other authors place the reign of this king 180 years earlier.

(e) PLIN. lib. iii. c. 5. JUSTIN. lib. xx. c. 5.

The motive of their flight, their civil deportment, and perhaps more so, the wealth they brought with them, procured them a favourable reception from the original inhabitants of that inhospitable region, who are mentioned by authors ^(f) as being a Celtic nation, fabulously conjectured from their name (*Λειποντιῶι*, *relicti*) to have been left there by HERCULES in his expedition into Spain.

The new adventurers had no sooner climbed over the highest precipices, but thinking themselves secure from the pursuits of their rapacious enemies, they fixed in a valley which, from its great fertility in comparison of the country they had just passed, they called *Domestica* ^(g). They intermixed with the old inhabitants, and built some towns and many castles, whose present names manifestly bespeak their origin ^(h). They soon after spread all over the country, which took the name of *Rhætia* from that of their leader; and introduced a form of government similar to their own, of which there are evident traces at this day, especially in the administration of justice; in which a *Laertes*, or President, now called Landamman, or Minister, together with twelve *Lucumones* ⁽ⁱ⁾, or Jurors determine, all causes, both civil and criminal ^(k): and

(f) CLUVER, Ital. Antiq. lib. i. c. 14.

(g) Probably by them pronounced *Tomiliafca*, the name it now bears.

(h) *Tusis* (Tuscia) and in Italian *Tosana*, the principal place; *Rhealta* (Rhetia alta); *Rheambs* (Rhetia ampla); *Rbazunz* (Rhetia ima), and above twelve other castles, the remains of which are now to be seen in the valley *Tomiliafca*.

(i) In some communities there are fourteen jurors besides the Landamman.

(k) SERV. in *Æneid*, lib. viii. 65. lib. x. 202. SPRECH. Pall. Rhæt. p. 9. SIML. Rep. Helv. p. 281. ed. 1735.

LIVY⁽¹⁾, although he erroneously pretends that they retained none of their ancient customs, yet allows that they continued the use of their language, though somewhat adulterated by a mixture with that of the Aborigines.

I must here interrupt the thread of this narration by observing, that the only way to account for the present use of a different language in the center and most craggy parts of the Grey League, is by allowing that the Tuscans, who, from the delicacy of their constitutions and habits, were little able, and less inclined, to encounter the hardships of so severe a climate and so barren a soil, never attempted to mix with the original and more sturdy inhabitants of that unfavoured spot; but left them and their language, which could only be a Celtic idiom, in the primitive state in which they found them^(m).

But to proceed;—several Roman families, dreading the fury of the Carthaginians under HANNIBAL; and perhaps, since during the rage of the civil wars, and the subsequent oppressive reigns, interior commotions and foreign invasions, forsook the Latium and Campania, and resorted for a peaceful enjoyment of their liberty, some into the islands where Venice now stands; and many into the mountains of the Grisons, where they chiefly fixed their residence in the Engadine⁽ⁿ⁾, as appears not only from the testimonies of authors^(o), but also

(1) LIV. lib. v. c. 33.

(m) SPRECH. p. 214. MER. l. c. .

(n) *En Code Ino*, perhaps the vulgar Roman phrase expressing *In Capite Ovis*. There are other etymologies, but all equally uncertain.

(o) SPRECH. p. 10.

from the names of several places and families which are evidently of Roman derivation^(p).

The inhabitants these emigrants found in that place of refuge could not but be a mixture of the Tuscans and original Lepontii: and the two languages which met upon this occasion must, at the very first, have had some affinity; as the Tuscan, which derived immediately from the Greek, is known to have had a great share in the formation of the Roman. But as it is generally observed, that the more polished people introduce their native tongue wherever they go to reside in any considerable numbers, the arrival of these successive colonies must gradually have produced a considerable change in the language of the country in which they settled^(q); and this change gave rise to the dialect since called Ladin, probably from the name of the mother country of its principal authors^(r).

Although the name of *Romanish*, which the whole language bears, seem to be a badge of Roman servitude, yet the conquest of that nation, if ever effected, could not have produced a great alteration in a language which must already have been so similar to their own; and its general name may as well be attributed to the pacific as

(p) *Lavin* (Lavinium), *Sus* (Susa), *Zernetz* (Cerneto), *Ardez* (Ardea), &c.

(q) SPRECH. p. 10.

(r) A parallel instance of the formation of a language by Roman colonies is the idiom of Moldavia; which, according to Prince CANTEMIR's account of that country, has still many traces of its Latin origin, and which, though engrafted upon the Dacian, and since upon the Sclavonian dialects of the Celtic, may still be considered as a sister language to that I am here treating of.

to the hostile Romans. But when we consider that a coalition of the two main dialects, which differ so far as not to be reciprocally understood, must have been the inevitable consequence of a total reduction; and that such a coalition is known never to have taken place, we may lay the greater stress upon the many passages of ancient authors⁽¹⁾, in which it is implied that the boasted victories of the Romans over the Rhæti, for which public honours had been decreed to L. MUNATUS, M. ANTHONY, DRUSUS, and AUGUSTUS, amounted to no more than frequent repulses of those hardy people into their mountains; out of which their want of sufficient room and sustenance, (which in our days drives considerable numbers of them into the services of foreign powers) compelled them at times to make desperate excursions in quest of necessities. And we may also from these collected authorities be induced to give the greater credit to the commentator of LUCAN⁽²⁾, and to the modern historians⁽³⁾, who positively assert, that the people living near the sources of the Rhine and the En were never totally subdued by the

(1) Videre Rhæti bella *sub* Alpibus

Drusum gerentem et Vindelici.

HOR. lib. 4. Od. iv.

———— immanesque Rhætos

Auspiciis *repulit* secundis.

Ibid. Od. xiv.

Fundat ab extremo flavos aquilone Suevos

Albis, et *indomitum* Rheni Caput.

LUC. lib. ii. 52.

———— Rhenumque minacem

Cornibus infractis.

CLAUD. Laud. Stilich. lib. i. 220.

(2) HORTEN. in LUCAN, p. 163. edit. 1578. fol.

(3) SPRECH. p. 18, &c.

Roman arms; but only repelled in their attempts to harraſs their neighbours.

This whole country, however, from its central ſituation, could not but be annumerated to one of the provinces of the empire; and accordingly we find that Rhætia itſelf (which by the accounts of ancient geographers (*) appears to have extended its limits beyond the lake of Conſtance, Augſburg, and Trent, towards Germany, and to Como and Verona towards Italy) was formed into a Roman province, governed by a pro-conſul or procurator, who reſided at Augſburg; and that when in the year 119, the Emperor ADRIAN divided it into Rhætia *prima* and *ſecunda*, the governor of the former, in which the country I am now ſpeaking of muſt have been comprized, took up his reſidence in two caſtles ſituated where Coire now ſtands, whiſt the other continued his ſeat at Augſburg. But notwithſtanding theſe appearances, no trace or monument of Roman ſervitude is to be met with in this diſtrict, except the ambiguous name of one mountain (y), ſituated on the ſkirts of theſe highlands, and generally thought to have been the *non plus ultra* of the Roman arms on the Italian ſide.

From the difficulty thoſe perſevering veterans experienced in keeping this ſtubborn people in awe, I mean to infer that ſuch ſtrenuous aſſertors of their independence, whom the flattering pens of OVID and HORACE repreſent as formidable even to AUGUSTUS, and prefer-

(*) STRABO, lib. IV. ſub fin. CLUVER, Ital. vet. lib. I. c. 16.

(y) *Julius Mœns*, SCHEUCHZER Iter. Alp. p. 114.

ring death to the loss of their liberties^(z), favoured by the natural strength and indigence of their country, were not very likely to be so far subdued by any foreign power inferior to the Roman, as to suffer any considerable revolution in their customs and language: for as to the irruptions of the Goths, Vandals, and Lombards, in the fifth and sixth centuries, besides a profound silence in history concerning any successful attempt of those barbarians upon this spot, it is scarce credible, that any of them should have either wished or endeavoured to settle in a country, perhaps far less hospitable than that they had just forsaken, especially after they had opened to themselves a way into the fertile plains of Lombardy.

Some stress must be laid upon this inference, as the history of what befel this country after the decline of the Roman empire is so intimately blended with that of Suabia, the Tyrolese, and the lower parts of the Grisons, which are known to have fallen to the share of the rising power of the Franks, that nothing positive can be drawn from authors as to the interior state of this small tract. The victory gained in the year 496 near Cologne, by CLOVIS I. king of the Franks, over the Almain, who had wrested from the Romans all their dominions on the Northern side of the Alps; and the defeat of both Romans and Goths in Italy, in the year 549, by the treacherous arms of THEODEBERT king of Austrasia, whose do-

(z) *Rætica nunc præbent Thraciæque arma metum.*

ovid. *Trist. lib. ii. 226.*

Devota morti pectora liberæ.

hor. 4 lib. *Od. xiv.*

minions soon after devolved to the crown of France, necessarily gave the aspiring Merovingian race a great ascendancy over all the countries surrounding the Grifons; and accordingly we find, that this district also was soon after, without any military effort, considered as part of the dominions of the reviving Western empire. But it does not appear that those monarchs ever made any other use of their supremacy in these parts than, agreeable to the feudal system which they introduced, to constitute dukes, earls, presidents and bailiffs, over Rhætia; to grant out tenures upon the usual feudal terms; and consequently to levy forces in most of their military expeditions.

It must, however, be observed, that these feudal substitutes were seldom, if ever, strangers: those who are upon record to the latter end of the eighth century having all been chosen from among the nobility of the country^(a). And that no foreign garrisons were ever maintained for any continuance of time in these parts, appears from a circumstance related by their annalists^(b); who say, that an inroad of the Huns in 670, when external forces would probably have been very acceptable to the natives, was repulsed merely by a concurrence of the inhabitants.

History continues to furnish us with proofs of the little connexion this people had with other nations in their domestic affairs, notwithstanding their dependance upon a foreign power. In the year 780, the Bishop of Coire, who by the constitution of that see can only be a na-

(a) SPRECH. p. 52—55.

(b) Ibid. p. 58.

tive^(c), obtained from CHARLEMAIN, besides many considerable honours and privileges in the empire, a grant of the supreme authority in this country, by the investiture of the office of hereditary president or bailiff over all Rhætia. His successors not only enjoyed this prerogative to the extinction of the Carlovingian race of emperors in 911, but received accumulated favours from other succeeding monarchs, as the bigoted devotion of those times or motives of interest prompted them. And so far did their munificence gradually extend, that the sole property of one of the three leagues^(d), was at one time vested in the hands of the bishop.

This prelate and the nobles, the greatest part of whom became his retainers, availed themselves, like all the German princes, of the confusion, divisions, and interreigns which frequently distracted the empire in the succeeding centuries, in order to establish a firm and unlimited authority of their own. Henceforth the annals of this country furnish us with little more than catalogues of the bishops and dukes, who were still, at times, nominated by the emperors; and of the domains granted out by them to different indigenate families; with accounts of the atrocious cruelties exercised by these lords over their vassals; and with anecdotes of the prowess of the

(c) This privilege hath at times been waved; but never without some plausible pretence, and a formal rescript, acknowledging the exclusive right.

(d) The League *Cadéa*, or of the *House of God*, so called from the cathedral of the bishopric of Coire, which is situated in its capital.

natives in several expeditions into Italy and Palestine, in which they still voluntarily accompanied the emperors.

The repeated acts of tyranny exercised by those arbitrary despots, who had now shaken off all manner of restraint, at length exasperated the people into a general revolt, and brought on the confederacy; in which the bishop and most of the nobles were glad to join, in order to screen themselves from the fury of the insurgents.

The first step towards this happy revolution was made by some *venerable old men dressed in the coarse grey cloth* of the country, who in the year 1424 met privately in a wood near a place called Truns, in the Upper League; where, *impressed with a sense of their former liberties*^(e), they determined to remonstrate against, and oppose, the violent proceedings of their oppressors. The abbot of Dissentis was the first who countenanced their measures; their joint influence gradually prevailed over several of the most moderate among the nobles; and hence arose the League which, from the colour of its first promoters, was ever after called the Grey League; which, from its being the first in the bold attempt to shake off the yoke of wanton tyranny, hath ever since retained the pre-eminence in rank before the two other leagues; and which hath even given its name to the whole country, whose inhabitants, from the circumstances of their deliverance, pride themselves in the appellation of *Grifons*,

(e) Canitie griseoque amictu venerandi.—Memores adhuc antiquæ libertatis.

*PRECH. p. 189.

or the *grey-ones* (*f*). From this period nothing hath ever affected their freedom and absolute independence; which they now enjoy in the most unlimited sense, in spite of the repeated efforts of the house of Austria to recover some degree of ascendancy over them.

From this concise view of the history of the Grifons, in which I have carefully guarded against favouring any particular hypothesis, it appears, that as no foreign nation ever gained any permanent footing in the most mountainous parts of this country since the establishment of the Tuscans and Romans, the language now spoken could never have suffered any considerable alteration from extraneous mixtures of modern languages. And to those who may object, that languages like all other human institutions will, though left to themselves, be inevitably affected by the common revolutions of time, I shall observe, that a language, in which no books are written, but which is only spoken by a people chiefly devoted to arms and agriculture, and consequently not cultivated by the criticisms of men of taste and learning, is by no means exposed to the vicissitudes of those that are polished by refined nations (*g*); and that, however paradoxical it may appear, it is nevertheless true, that the degeneracy of a language is more frequently to be attributed to an extravagant refinement than to the neglect of an

(*f*) The following barbarous distich is sometimes inscribed to the arms of the three Leagues,

Fœdera sunt cana, cana fidēs, cana libertas :

Hæc tria sub uno continentur corpore Rhæto.

(*g*) See Dr. PERCY's Preface to his Translation of MALLEY's Northern Antiquities, p. xxii. where this question is more amply discussed.

illiterate people, unless indeed external causes interfere. May we not hence conclude, that as the Romanish has never been used in any regular composition in writing till the sixteenth century, nor affected by any foreign invasion or intimate connexion, it is not likely to have received any material change before the period of its being written? And we have the authority of the books since printed to prove, that it is at present the identical language that was spoken two hundred years ago. These arguments will receive additional weight from the proofs I shall hereafter give of the great affinity there is between the language as it is now spoken, and the Romance that was used in France nine centuries ago.

When we further consider the facts I have above briefly related, the wonder will cease, that in a cluster of mountains, situated in the center of Europe, a distinct language (not a dialect or jargon of those spoken by the contiguous nations, as hath been generally imagined) should have maintained itself through a series of ages, in spite of the many revolutions which frequently changed the whole face of the adjacent countries. And indeed, so obstinately tenacious are these people of their independency, laws, customs, and consequently of their very language, that, as hath been already observed, their form of government, especially in judicial matters, still bears evident marks of the ancient Tuscan constitution; and that, although they be frequently exposed to inconveniences from their stubbornness in this respect, they have not yet been prevailed upon to adopt the Gregorian reformation of the calendar.

As to the nature of this language, it may now be advanced, with some degree of confidence, that the *Cialover* owes its origin to a mixture of the Tuscan and of the dialect of the Celtic spoken by the Lepontii; and that the introduction of the vulgar Roman affected it in some degree, but particularly gave rise to the *Ladin*; the vocabulary of which, as any one may be convinced by inspecting a few lines of the bible, hath a great affinity with that of the Latin tongue. But these assertions rest merely upon historical evidence; for as to the *Cialover*, all that it may have retained of the Tuscan or Roman, is so much disfigured by an uncouth pronunciation and a vague orthography, that all etymological enquiries are thereby rendered intricate and unsatisfactory. And as to the *Ladin*, although its derivation be more manifest, yet we are equally at a loss from what period or branch of the Latin tongue to trace its real origin; for I have found, after many tedious experiments, that even the vocabulary, in which the resemblance is most evident, differs equally from the classical purity of TULLY, CÆSAR, and SALLUST, as it does from the primitive Latin of the twelve tables, of ENNIUS, and the *columna rostralis* of DUILLIUS, which hath generally been thought the parent of the Gallic Romance; as also from the trivial language of VARRO, VEGETIUS, and COLUMELLA. May we not from this circumstance infer, that, as is the case in all vernacular tongues, the vulgar dialect of the Romans, the *sermo usualis, rusticus, pedestris*, ^(b) of which

(b) Conf. Mem. des Inscrip. tom. xxiv. p. 608.

there are no monuments extant, differed very widely both in pronunciation and construction from that which hath at any time been used either in writing or in the senate?

The grammatical variations, the syntax, and the genius of the language, must in this, as well as in several other modern European tongues, have been derived from the Celtic; it being well known, that the frequent use of articles, the distinction of cases by prepositions, the application of two auxiliaries in the conjugations, do by no means agree with the Latin turn of expression; although a late French academician⁽ⁱ⁾, who hath taken great pains to prove that the Gallic Romance was solely derived from the Roman, quotes several instances in which even the most classical writers have in this respect offended the purity of that refined language. It cannot here be denied, that as new ideas always require new signs to express them, some foreign words, and perhaps phrases, must necessarily, from time to time, have insinuated themselves into the Romanish by the military and some commercial intercourse of the Grisons with other nations; and this accounts for several modern German words which are now incorporated into the language of the Engadine^(k).

The little connexion there is in mountainous countries between the inhabitants of the different vallies, and the absolute independence of each jurisdiction in this

(i) BONAMY, v. *Mém.* des Inscrit. l. c.

(k) *Tapferdâ*, Tapferkeit, Bravery; *Nardâ*, Narheit, Folly; *Elinot*, Kleinod, a Jewel; *Graf*, Graf, a Count; *Baur*, Baur, a Peasant, &c.

district, which still lessens the frequency of their intercourse, also accounts, in a great measure, for the variety of secondary dialects subsisting in almost every different community or even village.

The oldest specimens of writing in this language are some dramatical performances in verse upon scriptural subjects; which are extant only in manuscript. The histories of SUSANNA, of the Prodigal Son, of JUDITH and HOLOFERNES, and of ESTHER, are among the first; and are said to have been composed about the year 1560. The books that have since been printed are chiefly upon religious subjects; and among those that are not so, the only I have ever heard of are a small code of the laws of the country in the Cialover dialect, and an epitome of SPRECHER'S Chronicle, by DA PORTA, in the Ladin.

The language spoken in Gaul from the fifth to the twelfth centuries being evidently a mixture of the same Roman and Celtic ingredients, and partaking of the same name with those of the Grisons; it will, I hope, not be thought foreign to the subject of this letter, if I enter into a few particulars concerning it, as it seems to have been an essential part, or rather the trunk, of the language, the history of which I am endeavouring to elucidate.

One of the many instances how little the laboured researches of philologists into the origin of languages are to be depended upon, is the variety of opinions entertained by French authors concerning the formation of

the Gallic Romance. A learned Benedictine ⁽¹⁾ first starts the conjecture, and then maintains it against the attacks of an anonymous writer, that the vulgar Latin became the universal language of Gaul immediately after CÆSAR'S conquest, and that its corruption, with very little mixture of the original language of the country, gradually produced the Romance towards the eighth century. BONAMY ^(m), on the other hand, is of opinion, that soon after that conquest, a corruption of vulgar Latin by the Celtic formed the Romance, which he takes to be the language always meant by authors when they speak of the *Lingua Romana* used in Gaul. The author of the Celtic Dictionary ⁽ⁿ⁾ tells us, that the Romance is derived from the *Latin*, the *Celtic*, which he more frequently calls Gallic, and the *Teutonic*; in admitting of which latter he deviates from most other authors ^(o), who deny that the Teutonic had any share in the composition of the Romance, since the Franks found it already established when they entered Gaul, and were long before they could prevail upon their new subjects to adopt any part of their own mother tongue, which however appears to have been afterwards instrumental in the formation of the modern French. DUCLOS ^(p), guided, I imagine, by DU CANGE ^(q), whose opinion appears to be

(1) RIVET, Hist. Litt. de la France, tom. vii. p. 1. et seq.

(m) Mem. des Inscrip. tom. xxiv. p. 594.

(n) BULLET, Mem. de la Langue Celtique, tom. i. p. 23.

(o) Mem. des Inscrip. tom. xxiv. p. 603.

(p) Ibid. tom. xv. p. 575. et seq.

(q) Præf. Gloss. n. xiii.

the most sober and best authenticated, maintains that the vulgar Latin was undoubtedly the foundation of the Romance; but that much of the Celtic gradually insinuated itself in spite of the policy of the Romans, who never failed to use all their endeavours in order to establish their language wherever they spread their arms.

Among this variety of conjectures and acute controversies, I find it however agreed on all hands, that the vocabulary of the Roman, and the idiom of the Celtic have chiefly contributed to the formation of the Gallic Romance, which is sufficient to prove that it partakes of a common origin with that of the Grisons.

There are incontestable proofs that this language was once universal all over France; and that this, and not immediately the Latin, hath been the parent of the Provençal, and afterwards of the modern French, the Italian, and the Spanish. The oath taken by LEWIS the Germanic, in the year 842, in confirmation of an alliance between him and CHARLES the Bald, his brother, is a decisive proof of the general use of the Romance by the whole French nation at that time, and of their little knowledge of the Teutonic, which being the native tongue of LEWIS, would certainly have been used by him in this oath, had it been understood by the French to whom he addressed himself. But NITHARDUS^(r), a contemporary writer and near relation to the contracting parties, informs us, that LEWIS took the oath in the Romance language, in order that it might be understood by

(r) DU CHESNE, Hist. Franc. tom. ii. p. 374.

the French nobility who were the subjects of CHARLES; and that they, in their turn, entered into reciprocal engagements in *their own language*, which the same author again declares to have been the Romance, and not the Teutonic; although one would imagine that, had they at all understood this latter tongue, they could not but have used it upon this occasion, in return for the condescension of LEWIS.

As a comparison between this language and the Romanish of the Grifons cannot be considered as a mere object of curiosity; but may also serve to corroborate the proofs I have above alledged of the antiquity of the latter, I have annexed in the appendix^(*), a translation of this oath into the language of Engadine, which approaches nearest to it; although I must observe, that there are in the other dialect some words which have a still greater affinity with the language of the oath, as appears by another translation I have procured, in which both dialects are indifferently used. To prevent any doubts concerning the veracity of these translations, I must here declare, that I am indebted for them, and for several anecdotes concerning that language, to a man of letters, who is a native and hath long been an inhabitant of the Grifons, and is lately come to reside in London. I have added to this comparative view of those two languages, the Latin words from which both seem to have been derived; and, as a proof of the existence of the Gallic Romance in France down to the twelfth century, I have

(*) N° I.

also subjoined the words used in that kingdom at that period, as they are given us by the author of the article (*Langue*) *Romane*, in the French Encyclopedie.

To the comparison of the two Romances, and the similarity of their origin, I may now with confidence add the authority of FONTANINI⁽¹⁾ to prove, that they are one and the same language. This author, speaking of the ancient Gallic Romance, asserts that it is now spoken in the country of the Grisons; though, not attending to the variety of dialects, some of which have certainly nothing of the Italian, he supposes it to have been altogether adulterated by a mixture of that modern tongue.

Whilst the Grisons neglected to improve their language, and rejected, or indeed were out of the reach of every refinement it might have derived from polished strangers, the taste and fertile genius of the Troubadours, fostered by the countenance and elegance of the brilliant courts and splendid nobility of Provence, did not long leave theirs in the rough state in which we find it in the ninth century. But the change having been gradual and almost imperceptible, the French historians have fixed no *epocha* for the transition of the Romance into the Provençal. That the former language had not received any considerable alteration in the twelfth century may be gathered from the comparison in the appendix: and that it still bore the same name, appears from the titles of several books which are said to have been written in, or translated into, the Romance. But though mention is made

(1) Eloq. Ital. p. 44.

of that name even after this æra, yet upon examining impartially what is given us for that language in this period, it will be found so different from the Romance of the ninth century, that to trace it any further would be both a vain and an extravagant pursuit.

Admitting, however, the universal use of the Romance all over France down to the twelfth century, which no French author hath yet doubted or denied; and allowing that what the writers of those times say of the Gallic is to be understood of the Romance, as appears from chronological proofs, and the expressions of several authors prior to the fifth century^(u); who, by distinguishing the *Gallic* both from the *Latin* and the *Celtic*, plainly indicate that they thereby mean the Romance, those being the only three languages which, before the invasion of the Franks, could possibly have been spoken, or even understood in Gaul: admitting these premises, I say, it necessarily follows, that the language introduced into England under ALFRED, and afterwards more universally established by EDWARD the Confessor, and WILLIAM the Conqueror, must have been an emanation of the Romance, very near akin to that of the abovementioned oath, and consequently to that which is now spoken in the Alps.

The intercourse between Britain and Gaul is known to have been of a very early date; for even in the first

(u) Fidei commissa quocunque Sermonem relinqui possunt, non solum *Latino* vel *Græco*, sed etiam *Punico* vel *Gallicano*. Digest. l. xxii. tit. i. § 11.

Tu autem vel *Celtice*, vel si mavis *Gallice*, loquere. SULP. SEV. Dial. i. § 6. sub fin.

century we find, that the British lawyers derived the greatest part of their knowledge from those of the continent^(*); whilst, on the other hand, the Gallic Druids are known to have resorted to Britain for instruction in their mysterious rites. The Britons, therefore, could not be totally ignorant of the Gallic language. And hence it will appear, that GRIMBALD, JOHN, and the other doctors, introduced by ALFRED^(y), could find no great difficulty in propagating their native tongue in this island; which tongue, at that interval of time, could only be the true Romance, since they were cotemporaries with LEWIS the Germanic.

That the Romance was almost universally understood in this kingdom under EDWARD the Confessor, it being not only used at court, but frequently at the bar, and even sometimes in the pulpit, is a fact too well-known and attested^(z) to need my further authenticating it with superfluous arguments and testimonies.

DUCLOS, in his History of the Gallic Romance^(a), gives the abovementioned oath of LEWIS as the first monument of that language. The second he mentions is the code of laws of WILLIAM the Conqueror^(b), whom the least proficient in the English history knows to have rendered his language almost universal in this kingdom.

(*) Gallia Caufidicos docuit facunda Britannos. JUV. Sat. xv. 111.

(y) WILLIAM of Malmsb. l. ii. c. 4.

(z) INGULPH. passim. DU CHESNE, tom. iii.

(a) Mem. des Inscrit. tom. xvii. p. 179.

(b) WILKINS, Leges Anglo-Sax.

How little progress it had yet made towards the modern French; and how great an affinity it still bore with the present Romansh of the Grisons, will appear from the annexed translation of the first paragraph of these laws into the latter tongue (c).

If we may credit DU CANGE (d), who grounds his assertion upon various instruments of the kings of Scotland during the twelfth century, the Romance had also penetrated into that kingdom before that period.

The same corruption, or coalescence, which gave rise to the Gallic Romance, and to that of the Grisons, must also have produced in Italy a language, if not perfectly similar, at least greatly approaching to those two idioms. Nor did it want its Northern nations to contribute what the two other branches derived from that source (e). But be the origin what it will, certain it is, that a jargon very different from either the Latin or the Italian was spoken in Italy from the time of the irruptions of the barbarians to the successful labours of DANTE and PETRARCA; that this jargon was usually called the *vulgar idiom*; but that SPERONI (f), the father of Italian literature, and others, frequently call it the *common Italian Romance*. And if FONTANINI'S (g) authorities be sufficient, it appears that even the Gallic Romance, by the residence of the Papal Court

(c) Append. N° II.

(d) Pref. Gloss. n. xxi.

(e) FONTANINI, p. 4.

(f) SPERON. Dial. passim.—Conf. MENAGE, Orig. della Ling. Ital. voce ROMANZA.

(g) FONT. p. 17.

at AVIGNON, and from other causes, made its way into Italy before it was polished into the Provençal.

As to Naples and Sicily, the expulsion of the Saracens by the Normans, under ROBERT GUISCARD in 1059, must have produced in that country nearly the same effect, a similar event soon after brought about in England. And in fact we have the authority of WILLIAM of Apulia^(b) to prove, that the conquerors used all their efforts to propagate their language and manners among the natives, that they might ever after be considered only as one people. And HUGO FALCLAND^(c) relates, that in the year 1150, Count HENRY refused to take upon him the management of public affairs, under pretence of not knowing the language of the French; which, he adds, was absolutely necessary at court.

That the language of the Romans penetrated very early into Spain, appears most evidently from a passage in STRABO^(d), who asserts, that the Turditani inhabiting the banks of the Boetis, now the Guadalquivir, forgot their original tongue, and adopted that of the conquerors. That the Romance was used there in the fourteenth century appears from a correspondence between ST. VINCENT of Ferrieres and DON MARTIN, son of PETER the IVth of Arragon^(e); and that this language must once have been common in that kingdom appears manifestly from the present name of the Spanish, which

^(b) MYRAT. Script. Ital. tom. v. p. 255. ⁽ⁱ⁾ Ibid. tom. vii. p. 322.

^(k) Lib. iii.

^(l) MABILL. an. l. 64. n. 124.

is still usually called Romance^(m). These circumstances considered, I am not so much inclined to discredit a fact related by MABILLON⁽ⁿ⁾, who says, that in the eighth century a paralytic Spaniard, on paying his devotions at the tomb of a saint in the church of Fulda, conversed with a monk of that abbey, who, *because he was an Italian*, understood the language of the Spaniard. Neither does an oral tradition I heard some time ago appear now so absurd to me, as it did when it was first related to me, which says, that two Catalonians travelling over the Alps, were not a little surprized when they came into the Grisons, to find that their native tongue was understood by the inhabitants, and that they could comprehend most of the language of that country.

The universality of the Romance in the French dominions during the eleventh century, also accounts for its introduction in Palestine and many other parts of the Levant by GODFREY DE BOUILLON, and the multitude of adventurers who engaged under him in the Crusade. The assizes or laws of Jerusalem, and those of Cyprus, are standing monuments of the footing that language had obtained in those parts; and if we may trust a Spanish historian of some reputation^(o) who resided in Greece in the thirteenth century, the Athenians and the inhabitants of Morea spoke at that time the same language that was used in France. And there is great reason to ima-

(m) OROZCO, *Tes. Castell. voce ROMANCE*—Conf. CRESCIMB. *Volg. Poet. l. v. c. i.*

(n) *Act. Ben. Sæc. 3. p. 2. p. 258.*

(o) RAYM. MONTANERO *Crónica de JUAN I.*

gine, that the affinity the *Lingua Franca* bears to the French and Italian is intirely to be derived from the Romance, which was once commonly used in the ports of the Levant. The heroic atchievements and gallantry of the Knights of the Crofs also gave rise to the swarm of fabulous narratives; which, though not an invention of those days, were yet, from the name of the language in which they were written, ever after distinguished by the appellation of *Romances*(p).

I shall now conclude this letter by observing, that far from presuming that the Romance hath been preserved so near its primitive state only in the country of the Grisons, there is great reason to suppose that it still exists in several other remote and unfrequented parts. When FONTANINI informs us (q) that the ancient Romance is now spoken in the country of the Grisons, he adds, that it is also the common dialect of the Friulense, and of some districts in Savoy bordering upon the Dauphiné. And RIVET (r) seriously undertakes to prove, that the Patois of several parts of the Limousin, Quercy, and Auvergne (which in fact agrees singularly with the *Romanish* of the Grisons) is the very Romance of eight centuries ago. Neither do I doubt but what some inquisitive traveller might still meet with manifest traces of it in many parts of the Pyrenæans and other mountainous regions of Spain, where the Moors and other invaders have never penetrated. I have the honour to be, &c.

(p) HUET, Orig. des Rom. p. 126. ed. 1678.

(q) P. 43, 44.

(q) Hist. Litt. de la Fr. tom. vii p. 22.

A P P E N D I X.

N° I. Oath of LEWIS the Germanic.

1. Latin from which the Romances are derived. 2. Gallic Romance in which the oath was taken. 3. French of the twelfth century. 4. Romanish of Engadine, called Ladin. 5. Romanish of both dialects.

1. Pro Dei amore, et pro Christiano populo, et nostro
2. *Pro Deu amur, et pro Christian poblo, et nostro*
3. Por Deu amor, et por Christian pople, et nostre
4. *Per amur da Dieu, et per il Christian poevel, et noss*
5. Pro l'amur da Deus, et pro il Christian pobel, et nost

1. communi salvamento, de ista die in abante, in quan-
2. *commun salvament, d'ist di en avant, in quant*
3. commun salvament, de ste di en avant, en quant
4. *commun salvament, da quist di in avant, in quant*
5. commun salvament, d'ist di en avant, in quant

1. tum Deus sapere et posse mihi donat, sic salvabo ego
2. *Deus savir et podir me dunat, si salvarai io*
3. Deu faveir et pör me donne, si salvarai je
4. *Dieu favair et podair m'duna, shi salvaro ei*
5. Deus. favir et podir m'dunat, shi salvaro io

1. eccistum meum fratrem KARLUM, et in adjutum ero
2. *cist meon fradre KARLO, et in adjudab er*
3. cist mon frere KARLE, et en adjude ferai
4. *quist mieu frær CARLO, et in adgiud li faro*
5. quist meu fradr CARL, et in. adjudh faro

1. in quaque una cauſa, fic quomodo homo per directum
2. *in cadbuna coſa, ſi cum om per dreit*
3. en caſcune coſe, ſi cum om per dreict
4. *in cbiaduna cbioſſa, ſbi ſcho l'horn per drett*
5. in caduna coſa, ſi com om per drett

1. ſuum fratrem ſalvare debet, in hoc quod ille mihi
2. *ſon fardre ſakvar diſt, in o quid il me*
3. ſon frere ſalver diſt, en o qui il me
4. *ſieu frær ſakvar d'ueſs, in que chél a mi*
5. ſeu frad'r ſalvar deſs, in que chél me

1. alterum fic faceret; et ab Lothario nullum placitum
2. *altreſi fazet; et ab Ludber nul plaid*
3. altreſi faſcet; et a Lothaire nul plaid
4. *altreſi fadſcbefs; et da Lotbar mai non prendrò io un*
5. altreſi fazefs; et da Lothar nul plaid mai

1. nunquam prehendam quod meo volle ecciſti meo fratri
2. *nunquam prindrai qui meon vol ciſt meon fradre*
3. nonques prendrai qui par mon voil a ciſt mon frere
4. *plæd che con mieu volair a quiſt mieu frær*
5. non prendrò che con meu voler a quiſt meu frad'r

1. KARLO in damno ſit.
2. KARLE *in damno ſit.*
3. KARLE en dam ſeit.
4. CARLO *ſai in damn.*
5. CARL in damn ſia.

N° II. The first Paragraph of the Laws of WILLIAM the Conqueror.

1. The Latin translation. 2. The French original. 3. A translation into the Romanish of both dialects.

1. Hæ sunt Leges et Consuetudines quas WILLELMUS Rex

2. *Ce sont les Leis et les Custumes que li Reis WILLIAM grantut*

3. Que sun las Leias e'ls Custums que il Rei WILLELM ga-

1. concessit toto populo Angliæ post subactam terram.

2. *a tut le peuple de Engleterre après le conquest de la terre.*

3. rantit a tut il poevel d'Engelterra dapo il conquist della

1. Eædem sunt quas EDWARDUS Rex Cognatus ejus obser-

2. *Ice les meismes que le Reis EDWARD sun Cofin tint*

3. terra. E sun las medemas que il Rei EDWARD seucuftrin

1. vavit ante eum. Scilicet: Pax Sanctæ Ecclesiæ,

2. *devant lui. Co est a faveir: Pais a Sainte Eglise,*

3. tenet avant el. Co es da favir: Pæsh alla Santa Ba-

1. cujuscunque forisfacturæ quis reus fit hoc tempore, et

2. *de quel forfait que home out fait en cel tens, et*

3. *felga*^(a), da quel sfarfatt que om a fatt en que temp, et

1. venire potest ad sanctam Ecclesiam, pacem habeat vitæ

2. *il pout venir a sainte Eglise, out pais de vie*

3. *il pout venir alla Santa Baselga, haun pæsh da vitta*

(a) The word *Ecclesia* being more modern in the Latin tongue than *Basilica*, the Romanish word *Baselga* derived from the latter is an additional proof of the antiquity of this language.

1. et membri. Et ſi quis injecerit manum in eum qui
2. *et de membre. E ſe alquons meift main en celui qui*
3. et da members. E ſi alcun metta man a quel que l'a

1. matrem Eccleſiam quæſierit, ſive ſit Abbatia five
2. *la mere Eglife requireit, ſe ceo fuſt u Abbeie u*
3. mamma Baſelga requira, qu'ella fuſs Abbatia u

1. Eccleſia religionis, reddat eum quem abſtulerit et
2. *Eglife de religion, rendiſt ce que il javereit pris*
3. Baſelga da religiun, renda que qu'el favares prais, et

1. centum ſolidos nomine forisfacturæ, et matri Eccleſiæ
2. *e cent ſols de forfait, e de Mer Eglife de*
3. cent folds da ſfarfatt, et alla mamma Baſelga da

1. parochiali 20 ſolidos, et capellæ 10 ſolidos: Et qui fregerit
2. *paroiffe 20 folds, e de Chapelle 10 folds: E que enſraiant*
3. parochia 20 folds, e da capella 10 folds: E que infrignand

1. pacem Regis in Merchenelæ 100 ſolidis emendet;
2. *la pais le Rei en Merchenelæ 100 folds les amendes;*
3. la pæſhr del Rei in Merchenelæ 100 folds d'amenda;

1. ſimiliter de compenſatione homicidii et de infidiis
2. *altrefi de Heinfare e de arweit*
3. altrefi della compenſatiun del omicidii et infidias

1. præcogitatis.
2. *purpenſed.*
3. perpenſadas.

VIII. *A Supplement to a Paper, entitled, Observations on the Population of Manchester*^(a). By Thomas Percival, M. D. F. R. and A. S.

TO THE REV. DR. HORSLEY.

REV. SIR,

R. Dec. 14,
1775. **A** PAINFUL nervous complaint in my eyes, with which I have been troubled a few days, lays me under the necessity of writing to you by an *amanuensis*. I beg leave to return you my best thanks for your kind attention to my Memoir. If you think the following additions of importance, you are at liberty to annex them to it^(b).

I have the honour to be, &c.

FROM the table of the number of males and females baptized in different places it appears^(c), that the proportion of males to females baptized is nearly as 13 to 12, agreeable to the calculation of Mr. DERHAM; but the succeeding ones shew, the number of females alive considerably exceed the number of males, in a variety of places; and that the widows are almost double the number of widowers.

(a) See Philosoph. Transactions, vol. LXV. art. xxiii.

(b) These papers came too late to my hands to be inserted in the last publication. S. HORSLEY.

(c) See Philosoph. Transf. vol. LXV. p 333.

A comparative view of the numbers of males and females
in different places.

Places.	Males.	Females.
Manchester,	10548	11933
Salford,	2248	2517
Townships of ditto,	947	958
Parish of Manchester,	6942	6844
Bolton,	2159	2392
Little Bolton,	361	410
Monton,	196	190
Hale,	140	136
Horwich,	149	136
Darwen,	900	959
Cockey,	320	391
Chowbent,	554	606
Ackworth,	340	388
Eaftham,	451	461
Chinley,	181	168
Brownfide,	40	47
Bugsworth,	80	95
Ashton under Line,	1406	1453
Parish of ditto,	2584	2513
Tattenhall parish,	382	399
Waverton parish,	310	332
Total,	31238	33339

A comparative view of the number of widowers and widows in different places.

Places.	Widowers.	Widows.
Manchester,	432	1064
Salford,	89	149
Township of ditto,	21	42
Parish of Manchester,	232	315
Monton,	14	13
Hale,	8	12
Horwich,	9	8
Darwen,	30	48
Cockey,	10	27
Chowbent,	26	43
Chinley, Brownside, and Bugfworth,	15	18
Ashton under Line,	50	81
Parish of ditto,	67	95
Total,	<hr/> 1003	<hr/> 1915

Let no arguments in favour of polygamy be drawn from these tables. The practice is brutal; destructive to friendship and moral sentiment; inconsistent with one great end of marriage, the education of children; and subversive of the natural rights of more than half of the species

— “Higher of the genial bed by far.

“And with mysterious reverence I deem.” MILTON.

Nor

Nor is this tyranny of man over the weaker, but more amiable sex favourable to population. For notwithstanding the number of females in the world may considerably exceed the number of males, yet there are more men capable of propagating their species, than women capable of bearing children. This painful office gradually becomes more dangerous and less frequent as the rigidity of the fibres increases, and ceases entirely at the age of fifty. The fatality of it is thus wisely obviated, and the comforts of declining life are not interrupted by the arduous toil of nursing. An institution, therefore, which confines in servile bondage to one usurper, many females in the prime of youth, must leave numbers destitute of the means, which nature hath pointed out, for perpetuating and increasing the race of mankind. And it is a fact well known, that Armenia, in which a plurality of wives is not allowed, abounds more with inhabitants than any other province of the Turkish empire.

P. S. Since the preceding paper was written, the rev. Mr. CRADDOCK hath favoured me with a survey of the town and parish of Ashton under Line, distant about eight miles from Manchester. The inhabitants consist of manufacturers and farmers.

An enumeration of the inhabitants of the town and parish
of Ashton under Line, made in 1775.

	Town.	Parish.
Inhabitants,	2859	5097
Houses,	553	941
Families,	599	971
Males,	1406	2584
Females,	1453	2513
Married,	982	1679
Widowers,	50	67
Widows,	81	95
Under five years of age,	509	896
From five to ten,	396	764
ten to twenty,	541	1011
twenty to fifty,	1044	1882
fifty to seventy,	307	471
seventy to ninety,	62	73

The rev. Dr. PEPLOE, chancellor of the diocese of Chester, has honoured me with the following account of the parishes of Waverton and Tattenhall, both in the neighbourhood of Chester. The inhabitants are farmers and labourers.

An enumeration of the inhabitants of Tattenhall, made
in August, 1774, by the rev. BRICE STORR, curate.

Inhabited houses,	148
Uninhabited ditto,	2
Heads of families,	176
Aged above 14 years,	462
Men and boys,	382
Women and girls,	399

	Christenings.	Burials.
1764	28	8
1765	21	9
1766	19	12
1767	29	11
1768	28	16
1769	24	15
1770	37	15
1771	30	9
1772	26	15
1773	38	20
	<hr/>	<hr/>
	280	130

An enumeration of the inhabitants of Waverton, made in August, 1774, by the rev. Mr. BISSETT, minister of the parish.

Inhabited houses,	109
Uninhabited ditto,	2
Heads of families,	116
Aged above 14 years,	406
Men and boys,	310
Women and girls,	322

	Christenings.	Burials.
1764	19	10
1765	26	2
1766	17	7
1767	18	10
1768	22	10
1769	17	7
1770	20	8
1771	23	9
1772	18	12
1773	13	9
	<hr/>	<hr/>
	193	84
	<hr/>	<hr/>

In the valuable work which I have so often quoted, Dr. PRICE hath given many convincing and melancholy proofs of the declining state of population in this kingdom. The growth of large towns; the prevalence of
vice

vice and luxury; the discouragements to marriage; the destruction of cottages; and various other causes have the most unfavourable influence on the increase of mankind. But it is to be hoped, that these evils do not universally prevail; and that even some good may arise from them to check their baneful effects. Certain it is, that in this part of England the inhabitants multiply with great rapidity: and though the increase may be chiefly owing to recruits drawn from other counties, yet the flourishing state of our manufactures cannot fail to promote population, by affording plentiful means of subsistence to the poor. The bishop of Chester informs me, that in various parish registers which he has consulted, the births have progressively become more numerous from generation to generation. At Boxley in Kent, where his lordship was vicar, he divided the times, from the commencement of the reign of queen ELIZABETH, into periods of twenty-one years; and found, that the number of births in the first period was 310, and in the last 525. The increase was gradual through the whole time.

Manchester,
Sept. 20, 1775.

IX. *Violent Asthmatic Fits, occasioned by the Effluvia of Ipecacoanha.* By William Scott, M. D. of Stamfordham, Northumberland.

R. Dec. 21, 1775. **M**R S. S. of Stamfordham, in Northumberland, married a person of the medical faculty in the year 1759, being then about twenty-six years of age. She had been always remarkably healthy before that period, and quite free from all nervous or other complaints, except a trifling nervous head-ach that used to affect her temples and forehead, sometimes for a night or so, about the time of her menstruation. The first year or two after marriage she enjoyed her usual good health and spirits in general; but at times she was afflicted with a very troublesome shortness of breathing, attended with a remarkable stricture about her throat and breast, and with a particular kind of wheezing noise. These fits came on very suddenly, and without any previous cause that at first could be assigned; and were often so violent as to threaten immediate suffocation. The duration of them was uncertain, sometimes longer and sometimes shorter; but in general they went off in two or three days, and commonly with a spitting of a tough phlegm, which, she said, had a disagreeable metallic taste. When these fits were off, she enjoyed her usual good health and spirits: had

children; but suffered as little as any woman could do, either in breeding or lying-in; and it was not observed, that she was more subject to these fits when with child than at other times. She was blooded, and took some common pectoral medicines for them; but without any benefit. About a year and a half, or two years, after her marriage, she told her husband, that she observed these fits always attacked her when any *Ipecacuanha* was powdered in his shop; and that she was certain the effluvia of that medicine immediately brought them on. This was looked upon at first as a fancy, and little regard paid to it for some time. However, frequently after this, when any of that medicine was powdering or putting up, she used immediately to call out, perhaps from a different room, that she found the *ipecacuanha*, and that they would see her immediately affected by it. This I, and several others, saw frequently happen as she had said; so that we were at last convinced, to a demonstration, that the effluvia of the medicine, some how or other, so affected her nerves, as to bring on a very great and remarkable degree of spasm all about her throat and breast. Having thus had several repeated proofs of the effects the medicine had upon her, great precaution was therefore taken for several years never to pound any of it, but to purchase it powdered; and also care was taken, when weighing or putting any of it up, to send her out of the way, or to some distant part of the house. By these means she was kept pretty clear of it for seven or eight years together, during which time she enjoyed perfect good health.

Betwixt nine and ten o'clock in the evening, June 3, 1775, her husband happening to have got a quantity of the *pulv. ipecacoanha* home, without considering opened it out, and put it into a bottle: his wife not being far off at the time, and then in perfect good health, immediately, almost even before it was got quite into the bottle, called out that she felt the *ipecacoanha* affect her throat; on which she was immediately seized with the stricture upon her breast and difficulty of breathing. She was advised to walk out into the air, to try if that would remove it; but it had little or no effect. She went to bed some little time afterwards; was exceedingly ill all night; and betwixt two and three o'clock next morning (June 4th) I saw her, when she was gasping for breath at a window, was as pale as death, her pulse scarce to be felt, and in short seemed evidently to be in the utmost danger of suffocation. She had seven or eight ounces of blood taken from her arm, her feet put into warm water, an anodyne draught with seven or eight drops of *laudanum* given her, and took frequently a table spoonful of oil of almonds. None of these seemed to have the least effect: and she continued much in the same way, with few or no intervals of ease, till about nine o'clock that morning; when, being in a manner almost exhausted, she fell into a kind of disturbed sleep, the difficulty of breathing with a wheezing noise still continuing with little abatement. She slept some little time, and got out of bed again about eleven o'clock that forenoon; her breathing still very difficult, and her eyes looked red and

a little inflamed. After she got up, she became easier towards the afternoon, and it was then supposed it would go off. Dr. BROWN, an eminent physician of Newcastle upon Tyne, happening to be in the neighbourhood, called upon Mrs. S.; and being told what had happened, said he had known a case, pretty much similar, from the same cause; and hoped, as she then seemed better, it would soon go off; recommended to her riding out as soon she was able, and to be kept open. Towards bedtime the same evening (June 4th) the difficulty of breathing returned, and she was again exceedingly ill all night; had flannel cloths wrung out of warm water applied to her feet, breast, and throat, with little or no advantage; was blooded again about four o'clock next morning (June 5th), and had also a blister applied to the back part of her neck, still continuing to take now and then a spoonful of the oil of almonds. She again fell into some sleep about nine in the morning, and continued in bed till betwixt eleven and twelve: got up, and was again a little easier during the day; but at night was as bad as ever. And the same scene was continued for eight days and nights successively; that is, she was generally a little easier from about eleven o'clock of the forenoon, although still far from well, till towards ten or eleven at night, when the shortness of breathing always returned very violently. However, after eight days she began to get better rest at nights; the asthmatic fits were neither so long nor so violent; and in about fourteen days from the accident were almost entirely

gone off; and at the writing of this letter, August 1, 1775, although she is now in very good health, she has not yet quite recovered her usual flesh, strength, and colour. Besides the abovementioned medicines, she took at times, during the first eight days, small quantities of an emulsion of *sperma ceti*, *lac. ammoniac.* and *succ. liquorit.*; had a dose of cooling physic; rode and walked out a little sometimes; had a few anodyne draughts with seven or eight drops of *laudanum*; but it could not be observed that she got any benefit from them, except that she sometimes thought the oil of almonds gave her a little ease. She had a slight appearance of the *menfes* about four or five days after the accident happened, although it was then only about the middle of the usual period; coughed up at times some small quantities of blood, and had also some mixed with her stools and urine. The reason why the *laudanum*, the most effectual and universal anti-spasmodic, was used in such small quantities, was, that it was known before that, she could never bear above eight or nine drops of it, as the common dose used to affect her with violent sickness at stomach, giddiness and pain in her head, &c. to so great a degree, that for some years past, she neither would take, nor durst her husband administer, a larger dose to her. At the time the above accident happened, she was neither with child, nor had had any for some years before.

The above effects of *ippecacoanba*, I believe, very seldom happen, and no doubt arise from some peculiarity of constitution. Medical writers, at least as far as I can recollect

recollect at present, seem to have taken little or no notice of its ever producing such an effect as the above. QUINCEY, however, if I remember right, mentions its producing asthmas; but then he seems to mean, that it has that effect sometimes when taken internally, but not by means of its effluvia.

Mr. LEIGHTON, a reputable surgeon and apothecary in Newcastle, told me, that the effluvia of *ippecacoanha* had the very same effect upon his wife, as it is above described to have had upon Mrs. S.; and that he had once, in particular, very near lost her from having some of it powdered in his shop.

The *ippecacoanha* which had the above effects upon Mrs. S. was the common officinal ash-coloured or grey kind.

Mrs. S. has now (Oct. 20, 1775) quite recovered her flesh, strength, colour, &c. I have sometimes thought since, that perhaps musk in pretty large doses might have been of service to her.

X. *An Account of the Success of some Attempts to freeze Quicksilver, at Albany Fort, in Hudson's Bay, in the Year 1775: with Observations on the Dipping-needle.*
By Thomas Hutchins, Esquire, in a Letter to Dr. Maty, Sec. R. S.

T O ' D R. M A T Y.

S I R,

Albany Fort, Hudson's Bay,
August, 28, 1775.

R. Dec. 21, 1775. **I** H A D made two or three more observations on the dipping-needle, but a violent fit of sickness, which hath for some time attended me, and the having mislaid many of my papers oblige me to send only these few to the Royal Society. I can assure you they were made with all possible circumspection. I have sent home the instruments by Captain CHRISTOPHER as desired, and inclose his receipt for them. The meteorological remarks have been made at all the places within my jurisdiction. I am very sorry not to be able this year to send home those from Albany, because time will not permit me to copy them fair, and the ill state of my health prevents it being done sooner; but they shall certainly be sent next year. The snow in my receiver during the whole winter was $64\frac{1}{2}$ inches.

Agreeable

According to the instructions you were so obliging to give me, concerning the congelation of quickfilver by cold, I made my first attempt to produce that extraordinary phænomenon on the 19th January, 1775. The thermometer at eight o'clock in the morning was at 37° below 0; but between ten and eleven it stood at 28° . I took the same thermometer and the best spare tube I had, which admitted only of 250° below 0, and immersed them both together in a large tea cup filled with snow, and poured on *sp. nitri fumans Glauberi* until the snow was dissolved; but finding it did not cover the bulbs, I added more snow and spirit until the bulbs were entirely covered in the mixture, which was now liquified: the quickfilver subsided very gradually to 130° , and then stopped. I had another cup at hand, and mixed some snow and spirit in it so as to liquify the mixture, and removed both the thermometers into it; but found the standard thermometer, by which I mean the instrument graduated by Mess. NAIRNE and BLUNT, London, had risen in the removal to 110° below 0. As the mixture in this second cup did not cover the bulbs, I added more as before, and also poured some out of the first cup. The spare tube, graduated by myself, stood in this cup at 130° ; but the standard fell deliberately to 263 , where it stood again. I therefore prepared a third cup as before; the quickfilver did not ascend in the removal, but when immersed it fell very swiftly: that in the spare tube sunk into the bulb, and that in the standard descended much quicker than before, until it came to 400° ; after,

after which it fell gently to 430° , and did not go beyond this point. As this was a greater degree of cold than that which Profeffor BRAUM faid quickfilver would freeze at, I determined to break my fpare tube, which was eafily done by a ftroke with a pair of fciffars; the quickfilver in a fall of about fix inches was flattened, and fome *globuli* appeared at the bottom of a tea cup in which it was received. This occafioned Mr. JARVIS the furgeon, who was fo obliging as to affift me, to exclaim the quickfilver was not frozen; but when he faw me repeatedly ftrike the cake with an hammer, and heard it give a deadifh found like lead, as M. BRAUM juftly expreffes it, he receded from his firft opinion. The quickfilver liquified in about fix or ten feconds. The furface, when frozen, was finely polished. I imagine the internal part of the globe was not frozen, and that the force of the fall having flattened it, might crack the external coat or fhell of congealed quickfilver, and permit the *globuli* which I faw to efcape into the cup. On taking the ftandard thermometer out of the mixture it fell 10° lower than when the bulb was immerfed; but it foon began to afcend, and being taken into my room, it rofe to 40 above 0, when I replaced it a little before noon. The operation lafted about thirty or forty minutes.

Having fucceeded thus far in my firft attempt to freeze quickfilver, I was anxious for another opportunity; but fometimes bufinefs, and want of a fufficient degree of cold in the air at other times, obliged me to defer it until the 11th of February, which was very clear, and the
ther-

thermometer stood at 36° below 0. I began the operation exactly at forty-five minutes past eight: the instrument being at 28° was put into a large tea-cup with the mixture as above, together with a spare tube, graduated by myself; the quicksilver in the latter subsided into the bulb, which was only 200° below 0; in the standard thermometer it sunk to 447° at fifty-nine minutes after eight o'clock, which gave me great hopes of succeeding still better than in my first attempt, because I had now a greater degree of cold in my first cup than I had before in my third. Finding it did not go any lower, I removed it into a second cup, prepared as before; but the quicksilver shewed no alteration in it. After waiting a considerable time, I removed it into a third; but in the removal, the quicksilver rose to 380° below 0. I imagined I had put in too much spirit in proportion to the snow, and therefore added more of the latter, by which means it subsided to 408° ; and after standing at this point for some time, it rose to 406° ; and soon after, at ten minutes after nine o'clock, it rose with great celerity and full of bubbles, until it came to 160° above 0, and in a minute after it reached the point of boiling water. On examining the instrument, I found the bulb cracked and the quicksilver fluid, to my surprize and regret.

R E M A R K S.

I imagine it is extremely difficult to ascertain the exact degree at which quicksilver begins to freeze, because no particular alteration or circumstance points out the

moment of congelation, or even afterwards; for the quicksilver in the tube still continues to fall, and hath the same appearance as before, contrary to what we observe in water. I think, therefore, it can only be determined by breaking the glasses at different altitudes; but this would be both tedious and expensive. However, were spare tubes filled by the maker, and graduated by the operator, to be made use of, the expence would be less; but then, if those tubes will not admit of being graduated to a considerable distance (suppose 1000°) below 0, the operator is obliged to put a thermometer, with a scale graduated by the instrument-maker, together with the other tube, into the mixture, to learn the degree of cold after the quicksilver in the spare tube, designed chiefly for the experiment, hath subsided into the bulb, as was my case. Professor BRAUM made it subside even to 1500° , which shews the fineness of the tubes he made use of.

These, SIR, are all the observations that much business, and an infirm state of health, permit me at this time to transmit to you. I wish they were more worthy your attention. You are the best judge whether they will bear the inspection of the Royal Society, and to your candour I submit them. I propose making some more experiments this year, which I shall take a pleasure in communicating; being, with great regard, &c.

Observations on the Dipping-needle, at Albany Fort,
Longitude $83^{\circ} 30'$ West. Latitude $52^{\circ} 24'$ North.

February 3, 1775. .

76	50	Index East.
77	12	
80	0	Index West.
78	45	
79	20	changed the poles, the index still West.
79	47	
79	40	Index East.
79	34	
79	45	
79	17	
79	8	Index West.
78	50	
79	45	changed the poles again, the index West.
79	19	
81	8	Index East.
79	45	
81	25	Mean of all observations $79^{\circ} 17\frac{5}{8}$.

I took particular care in placing the instrument in the magnetic meridian, and was near four hours before I got it right. The observations employed me four hours more; and when they were finished, I turned the index South, when the needle pointed at $89^{\circ} 56'$, or very near perpendicular. I cannot possibly account for the differences, more especially as I took so much pains to render these observations correct. Ob-

March 13, 1775.

• /
77 45 Index East.

80 45

80 5

78 40

79 55 Index West.

80 6

79 45

80 8

78 10 Poles changed, and index West.

79 50

78 30

78 50

80 45 Index East.

78 45

80 15

78 20

Mean of the whole is $79^{\circ} 25\frac{1}{4}$.

May 6, 1775.

79 0 Index West.

78 0

79 45

79 15 Index East.

79 15

79 45

78 30 Poles changed, index still East,

80 35

80 10

79 45 Index West.

80 5

79 40

Mean $79^{\circ} 28',75$.

These observations were made in the open air, on a platform on the top of the fort.

XI. *Astronomical Observations made in the Austrian Netherlands in 1772 and 1773. By Nathanael Pigott, Esquire, F. R. S. Foreign Member of the Academies of Bruffels and Caen. In a Letter to the Reverend Nevil Maskelyne, Astronomer Royal, F. R. S.*

TO THE REV. NEVIL MASKELYNE.

REV. SIR,

Louvain,
August 21, 1775.

R. Dec. 9, 1775. **I** RECEIVED, about a month ago, the favour of your letter, and return you many thanks for the Greenwich observations, which you were so obliging to send me. I wait, with impatience, the publication of your journey into Scotland, which must be very curious and interesting. I beg of you, SIR, to present my respects to the Royal Society, with the inclosed astronomical observations, which I have contracted as much as I well could, consistently with a view of affording means to verify them, or rectify any mistake which, by inadvertency, may have crept in. I shall only add a short account of the instruments I used, and the elements I employed in the calculations, that a proper judgement may be formed, how far these observations may be depended upon.

The meridian altitudes were taken with a quadrant one foot radius made by Mr. BIRD, very steadily fixed; free from any communication with the floor, and well placed in the plane of the meridian.

Repeated observations for the error of the line of collimation gave $1' 58'',7$ additive to the zenith distances.

I always observed both the limbs of the Sun on the meridian, when the weather would permit: its declination was computed from the Nautical Almanac: its parallax and all refractions, account being always kept of the height of the barometer and thermometer, from Professor MAYER's tables, published by the Board of Longitude.

The declinations of the fixed stars were taken from the *Connoissance des tems*. The corrections on account of aberration and nutation, were either taken from the same ephemeris, or computed.

My son always observed 4's satellites with a reflector, made by SHORT, of eighteen inches focal length, magnifying 95 times. I observed them with a reflector of two feet and a half focal length, made by WING, magnifying the diameter of the object about 200 times.

The clock was a compound gridiron pendulum, made by the Sieur LE PAUTE at Paris.

The equal altitudes were taken with a quadrant of eighteen inches radius.

This astronomical journey was undertaken at the request of the government here. They expressed a desire that the situations of some of their towns, at least, should be determined by observation; and I readily concurred, without regretting either trouble or expence, in a project which had public utility in view.

I am, &c.

Cor-

Corresponding altitude of the Sun and Stars.

		h	'	"
1772				
Aug.	30. Clock at noon corrected by four obs. of Sun,	11	54	7,9
Sept.	5. Clock at noon corrected by seven ditto,	11	47	26,8
	13. Clock at noon corrected by four ditto,	0	2	48,5
	19. Clock at noon corrected by seven ditto,	11	57	21,1
	21. Clock at noon corrected by seven ditto,	11	54	42,7
Oct.	10. Clock at noon corrected by eight ditto,	11	32	37,7
	11. Clock at noon corrected by nine ditto,	11	31	20,0
	19. Clock at noon corrected by five ditto,	11	22	33,1
	20. Clock at noon corrected by five ditto,	11	21	31,6
	21. Clock at noon corrected by six ditto,	11	20	30,5+
Nov.	9. Clock at noon corrected by five ditto,	0	21	23,6—
	11. Clock at noon corrected by three ditto,	0	23	10+
	13. Clock at noon corrected by three sets ditto,	0	24	55,1
	14. Clock at noon corrected by six ditto,	0	25	9,7+
	20. Clock at noon corrected by seven ditto,	0	30	30,5—
	Ditto. Fomahant crossed the meridian by the clock,	7	28	28,0
	21. Fomahant crossed the meridian by ditto,	7	25	19,9—
Dec.	23. β Ceti on meridian by the clock,	6	39	38,5—
	24. Clock at noon corrected by four obs. of Sun,	0	19	6,0

N A M U R 1772.

By a mean of eight meridian altitudes of the fixed stars taken in September, I determined the latitude of my observatory, in the *Rue St. Nicholas*, near the Recollets Church, $50^{\circ} 28' 32''$ North.

Sept. 4, emerfion of α 's first fat. at $10^h 38'$ by the clock.

	Apparent times. h ' ''
Emerfion at Tyrnaw in Hungary, by Father WEISS, $3\frac{1}{2}$ feet achrom. } weather fine,	11 41 11
To reduce to time of the Royal Observatory at Paris,	—1 0 55
Emerfion of the fat. at Paris,	10 40 16
Emerfion by my fon at Namur,	11 49 55
Namur East of the Paris Observatory, in time,	9 39

Or in parts of a great circle $2^{\circ} 24' 45''$.

A very good obfervation. α 's belts very distinct.
I faw the emerfion $5''$ latter.

L U X E M B O U R G 1772.

By a mean of twenty-nine meridian altitudes of the Sun and fixed stars taken in September and October, one of which only, gives the latitude $19^{\circ}9'$ different from the mean of the whole, I determined the latitude of my observatory, in the *Rue St. Esprit*, near the Jesuits Church, $49^{\circ}37'6''+$ North,

Sept. 11. emerfion μ 's first fat. $12^h 55' 20''$ by the clock.

	Apparent times. h ' "
Emerfion at Greenwich, by the Nautical Almanac,	12 28 15
Error of tables, by a good observation at Tyraw, Sept. 4,	+ 0 7
Greenwich West of Paris,	+ 9 16
	<hr/>
Emerfion at Paris,	12 37 38
I observed it at Luxembourg,	12 53 19
	<hr/>
Luxembourg East of Paris Observatory,	15 41

My fon saw the emerfion $19''$ later.

LUXEMBOURG, Sept. 20, 1772.

Emerfion 4's firft fat. at 9h 15' 23" by the clock.

	Apparent times. h ' "
Emerfion at Greenwich, by Nautical Almanac,	8 55 12
Error of tables, as above,	+ 7
Greenwich Weft of Paris,	+ 9 16
Emerfion at Paris,	9 4 35
I obferved it at Luxembourg,	9 19 52
Luxembourg Eaft of Paris,	15 17

My fon obferved the emerfion 13" later.

The fame emerfion.

Emerfion at Greenwich, by Nautical Almanac,	8 55 12
Error of tables, by an obfervation at Greenwich on the 27th,	— 25
At Greenwich, by the tables corrected,	8 54 47
Difference of meridians,	+ 9 16
Emerfion at Paris,	9 4 3
I obferved it at Luxembourg,	9 19 52
Luxembourg Eaft of Paris,	15 49

LUXEMBOURG, Oct. 19, 1772.

Emerfion μ 's fecond fat. at 7h 31' 39" by the clock.

	Apparent time.
	h ' "
Emerfion at Tyrnaw, achrom. $3\frac{1}{2}$ feet,	8 54 47
To reduce to Paris time,	— 1 0 55
At Paris,	7 53 52
My fon obferved it at Luxembourg,	8 9 14
Luxembourg Eaft of Paris,	<hr/> 15 22
I faw it 3" later.	

The fame emerfion.

At Senones, by M. MESSIER, achrom. 5 feet,	8 12 8
To reduce to Paris time, according to his letter,	— 18 34
Emerfion at Paris,	7 53 34
At Luxembourg,	8 9 14
Luxembourg Eaft of Paris,	<hr/> 15 40

This emerfion was obferved alfo at 7h 44' 13" apparent time, with a 6 feet reflector, at Greenwich. Allowing, according to the Aftronomer Royal's rule, about 20" for the difference of telefcopes, the refult will be, Luxembourg Eaft of Paris 15' 25".

made in the Austrian Netherlands. 189

LUXEMBOURG, Oct. 20, 1772.

Emerfion 4's first fat. at 11h 0' 15" by the clock..

	Apparent time.
	h ' "
Emerfion at Greenwich, achrom. $3\frac{1}{2}$ feet,	11 14 18
To reduce to Paris time,	+ 9 16
	<hr/>
At Paris,	11 23 34
I obferved it at Luxembourg,	11 39 13
	<hr/>
Luxembourg Eaft of Paris,	15 39

My fon faw it 12" later..

LUXEMBOURG, Oct. 11, 1772.

Eclipse of the Moon.

By clock	Appar.					Luxemb.
	time.					East of
s	'	"	s	'	"	Paris.
6	25	18	6	54	17	
6	36	57	5	4		
6	38	07	6	59		
			6	51	37	15 22
			6	51	54	15 5
6	40	42	7	9	41	
6	42	35	7	11	34	
			6	56	13	15 21
			6	55	56	15 38
			6	56	21	15 13
6	53	56	7	22	54	
			7	7	25	15 29
			7	7	28	15 26
7	3	25	7	32	25	
			7	17	10	15 15
			7	17	23	15 2
7	12	17	7	41	17	
			7	26	18	14 59
7	17	37	7	46	35	
			7	30	45	15 50
By a mean,						
15 20+						
			7	52	7	
			7	33	55	
			7	35	46	
			7	34	54	

At Luxembourg, Sky remarkably clear, without the least wind.

Hence, by a mean of 14's satellites,

15 33+

Luxembourg East of the R. Observ. at Paris, by a mean of the whole, 15 27+

Which gives $3^{\circ} 51' 45''$ in parts of a great circle.

LUXEM

LUXEMBOURG, 1772.

Oct. 22. at 3 hours P. M. a magnetic needle of four inches, made by DOLLOND, gave the declination West $18^{\circ} 42\frac{1}{2}'$.

Oct. 23. at 10 hours A. M. the declination was $18^{\circ} 50'$.

At LA HEESE, near HOOGSTRAETEN.

By a mean of twenty-two meridian altitudes of the Sun and fixed stars taken in November 1772, one of which only, gives the latitude different from the mean of the whole $10''$, I determined the latitude of my observatory $51^{\circ} 23' 2'' + N$.

Nov. 9. Emerfion 4's third fat. 6h 49' 29" by the clock..

	Apparent time. h ' "
Emerfion at Tyndall, achrom. $3\frac{1}{2}$ feet,	7 49 2
To reduce to Paris time,	—1 0 55
Emerfion at Paris,	6 18 7
I observed it at La Heese,	6 27 30
La Heese East of the Paris Observatory,	9 43

My fon faw the Emerfion 13" later..

LA HEESE, NOV. 14, 1772.

Emerſion γ 's firſt fat. at 6h 44' 5" by the clock.

	Apparent time. h ' "
Emerſion at Senones, achrom. $3\frac{1}{2}$ feet,	6 27 7
To reduce to Paris time,	— 18 34
Emerſion at Paris,	6 8 33
I obſerved it at La Heſſe,	6 18 42
La Heſſe Eaſt of Paris,	10 9

The ſame emerſion.

At Greenwich, 6 feet reflector,	5 59 28
Difference of teleſcopes,	+ 0 15
Greenwich Weſt of Paris,	+ 9 16
Emerſion at Paris,	6 8 59
By my obſervation at La Heſſe,	6 18 42
La Heſſe Eaſt of Paris,	9 43

Nov. 20. Emerſion γ 's ſecond fat. at 8h 15' 59" by the clock.

Emerſion by my ſon, but I have no correſponding obſervation,	7 45 9
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At LA HEESE, Nov. 21, 1772.

Emerfion μ 's first fat. at 8h 45' 12" by the clock.

	Apparent time. h ' "
Emerfion at Greenwich, by Nautical Almanac,	7 53 54
Error of tables, by the obferv. at Greenwich, Senones, and Tyrnaw,	+ 12
Greenwich Weft of Paris,	+ 9 16
Emerfion at Paris,	8 3 22
I obferved it at La Heefe,	8 13 3
La Heefe Eaft of Paris,	9 41

My fon faw the emerfion 5" later.

By a mean of the obfervations of μ 's fatellites, *La Heefe* is Eaft of the Royal Obfervatory at Paris 9' 49" in time, or $2^{\circ} 27' 15''$. If the obfervation of Nov. 14, compared with that made at *Senones*, be rejected, the difference of meridians will be $7\frac{1}{2}''$ in time lefs.

At H O O G S T R A E T E N, Nov. 24, 1772.

By a base of 3028 feet, twice very exactly measured and angles taken with a quadrant 18 inches radius, I determined the church of *Hoogstraeten* 10380 feet North and 5873 feet East of the Observatory at *La Heese*.

Hence latitude of La Heese,	51° 23' 2"
Difference of latitudes,	+ 1 42
Latitude North of the church of Hoogstraeten,	51° 24' 44"
Longitude of La Heese, as above,	0° 9' 49"
Difference of meridians,	+ 6
Hoogstraeten East of the Royal Observatory at Paris, in time,	9 55
Or 2° 28' 45" in parts of a great circle.	

At O S T E N D E.

By a mean of 24 meridian altitudes of the Sun and stars taken in December, one of which only, gives the latitude 11,7 different from the mean of the whole, I determined the latitude of my observatory, in the *Rue de la Poste* 51° 15' 10" North.

The *Connoissance des tems* gives the lat. 51° 13' 55"; but I do not know in what part of the town, or by whom it was determined.

At

At OSTENDE, Dec. 23, 1772.

Emerfion μ 's first fat. at 4h 52' 48" by the clock.

	Apparent time. h ' "
Emerfion at Greenwich, by Nautical Almanac,	4 22 1
Error of tables, as Nov. 21,	+ 12
Greenwich West of Paris,	+ 9 16
Emerfion at Paris,	4 31 29
I observed it at Ostende,	4 34 2
Ostende East of Paris Observatory, in time,	2 33

Or 38' 15" in parts of a great circle.

Twilight strong; sky very clear and serene; good observation.

Dec. 24, at 3 hours P. M. I found the declination of the magnetic needle West $20^{\circ} 35\frac{1}{2}'$.

At TOURNAI, 1773.

By a mean of 14 meridian altitudes of the Sun and stars taken in January, one of which only gives the latitude $22''$, 8 different from the mean of the whole, I determined the latitude of my observatory in the *Rue des Jesuites* $50^{\circ} 36' 57''$ + North.

The weather would not permit to observe either μ 's satellites, or an occultation of a star by the Moon, for the longitude of Tournai.

XII. *An Account of some Attempts to imitate the Effects of the Torpedo by Electricity. By the Hon. Henry Cavendish, F. R. S.*

R. Jan. 18,
1775. **A**LTHOUGH the proofs brought by Mr. WALSH, that the phenomena of the torpedo are produced by electricity, are such as leave little room for doubt; yet it must be confessed, that there are some circumstances, which at first sight seem scarcely to be reconciled with this supposition. I propose, therefore, to examine whether these circumstances are really incompatible with such an opinion; and to give an account of some attempts to imitate the effects of this animal by electricity.

It appears from Mr. WALSH's experiments, that the torpedo is not constantly electrical, but hath a power of throwing at pleasure a great quantity of electric fluid from one surface of those parts which he calls the electrical organs to the other; that is, from the upper surface to the lower, or from the lower to the upper, the experiments do not determine which; by which means a shock is produced in the body of a person who makes any part of the circuit which the fluid takes in its motion to restore the equilibrium.

One of the principal difficulties attending the supposition, that these phenomena are produced by electricity, is, that a shock may be perceived when the fish is held under water; and in other circumstances, where the electric fluid hath a much readier passage than through the person's body. To explain this, it must be considered, that when a jar is electrified, and any number of different circuits are made between its positive and negative side, some electricity will necessarily pass along each; but a greater quantity will pass through those in which it meets with less resistance, than those in which it meets with more. For instance, let a person take some yards of very fine wire, holding one end in each hand, and let him discharge the jar by touching the outside with one end of the wire, and the inside with the other; he will feel a shock, provided the jar is charged high enough; but less than if he had discharged it without holding the wire in his hands; which shews, that part of the electricity passes through his body, and part through the wire. Some electricians indeed seem to have supposed that the electric fluid passes only along the shortest and readiest circuit; but besides that such a supposition would be quite contrary to what is observed in all other fluids, it does not agree with experience. What seems to have led to this mistake is, that in discharging a jar by a wire held in both hands, as in the above mentioned experiment, the person will feel no shock, unless either the wire is very long and slender, or the jar is very large and highly charged. The reason of which is, that metals conduct
fur-

surprizingly better than the human body, or any other substance I am acquainted with; and consequently, unless the wire is very long and slender, the quantity of electricity which will pass through the person's body will bear so small a proportion to the whole, as not to give any sensible shock, unless the jar is very large and highly charged.

It appears from some experiments, of which I propose shortly to lay an account before this Society, that iron wire conducts about 400 million times better than rain or distilled water; that is, the electricity meets with no more resistance in passing through a piece of iron wire 400,000,000 inches long, than through a column of water of the same diameter only one inch long. Sea-water, or a solution of one part of salt in 30 of water, conducts 100 times, and a saturated solution of sea salt about 720 times better than rain water.

To apply what hath been here said to the torpedo; suppose the fish by any means, to convey in an instant a quantity of electricity through its electric organs, from the lower surface to the upper, so as to make the upper surface contain more than its natural quantity, and the lower less; this fluid will immediately flow back in all directions, part over the moist surface, and part through the substance of its body, supposing it to conduct electricity, as in all probability it does, till the equilibrium is restored; and if any person hath at the time one hand on the lower surface of the electric organs, and the other on the upper, part of the fluid will pass through his body.

More-

Moreover, if he hath one hand on one surface of an electric organ, and another on any other part of its body, for instance the tail, still some part of the fluid will pass through him, though much less than in the former case; for as part of the fluid, in its way from the upper surface of the organ to the lower, will go through the tail, some of that part will pass through the person's body. Some fluid also will pass through him, even though he does not touch either electric organ, but hath his hands on any two parts of the fishes body whatever, provided one of those parts is nearer to the upper surface of the electric organs than the other. On the same principle, if the torpedo is immersed in water, the fluid will pass through the water in all directions, and that even to great distances from its body, as is represented in fig. 1. where the full lines represent the section of its body, and the dotted lines the direction of the electric fluid; but it must be observed, that the nearer any part of the water is to the fishes body, the greater quantity of fluid will pass through it. Moreover, if any person touches the fish in this situation, either with one hand on the upper surface of an electric organ, and the other on the lower, or in any other of those manners in which I supposed it to be touched when out of the water, some fluid will pass through his body; but evidently less than when the animal is held in the air, as a great proportion of the fluid will pass through the water: and even some fluid will pass through him, though he does not touch the fish at all; but only holds

his hands in the water, provided one hand is nearer to the upper surface of the electric organs than the other.

The second difficulty is, that no one hath ever perceived the shock to be accompanied with any spark or light, or with the least degree of attraction or repulsion. With regard to this, it must be observed, that when a person receives a shock from the torpedo, he must have formed the circuit between its upper and lower surface before it begins to throw the electricity from one side to the other; for otherwise the fluid would be discharged over the surface of the fishes body before the circuit was completed, and consequently the person would receive no shock. The only way, therefore, by which any light or spark could be perceived, must be by making some interruption in the circuit. Now Mr. WALSH found, that the shock would never pass through the least sensible space of air, or even through a small brass chain. This circumstance, therefore, does not seem inconsistent with the supposition that the phenomena of the torpedo are owing to electricity; for a large battery will give a considerable shock, though so weakly charged that the electricity will hardly pass through any sensible space of air; and the larger the battery is, the less will this space be. The principle on which this depends will appear from the following experiments.

I took several jars of different sizes, and connected them to the same prime conductor, and electrified them in a given degree, as shewn by a very exact electrometer; and then found how near the knobs of an instrument

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in the nature of Mr. LANE's electrometer must be approached, before the jars would discharge themselves. I then electrified the same jars again in the same degree as before, and separated all of them from the conductor except one. It was found, that the distance to which the knobs must be approached to discharge this single jar was not sensibly less than the former. It was also found, that the divergence of the electrometer was the same after the removal of the jars as before, provided it was placed at a considerable distance from them: from which last circumstance, I think we may conclude, that the force with which the fluid endeavours to escape from the single jar is the same as from all the jars together.

It appears, therefore, that the distance to which the spark will fly is not sensibly affected by the number or size of the jars, but depends only on the force with which they are electrified; that is, on the force with which the fluid endeavours to escape from them: consequently, a large jar, or a great number of jars, will give a greater shock than a small one, or a small number, electrified to such a degree, that the spark shall fly to the same distance; for it is well known, that a large jar, or a great number of jars, will give a greater shock than a small one, or a small number, electrified with the same force.

In trying this experiment, the jars were charged very weakly, inasmuch that the distance to which the spark would fly was not more than the 20th of an inch. The electrometer I used consisted of two straws, 10 inches long, hanging parallel to each other, and turning at one

end on steel pins as centers, with cork balls about $\frac{1}{4}$ of an inch in diameter fixed on the other end. The way by which I estimated the divergence of these balls, was by seeing whether they appeared to coincide with parallel lines placed behind them at about ten inches distance; taking care to hold my eye always at the same distance from the balls, and not less than thirty inches off. To make the straws conduct the better, they were gilded, which causes them to be much more regular in their effect. This electrometer is very accurate; but can be used only when the electricity is very weak. It would be easy, however, to make one on the same principle, which should be fit for measuring pretty strong electricity.

The instrument by which I found to what distance the spark would fly is represented in fig. 2.; it differs from Mr. LANE's electrometer no otherwise than in not being fixed to a jar, but made so as to be held in the hand. The part ABCDEFGKLM is of baked wood, the rest of brass; the part GKL being covered with tinfoil communicating with the brass work at FG; and the part ABM being also covered with a piece of tinfoil, communicating with the brass work at CD.

I next took four jars, all of the same size; electrified one of them to a given degree, as shewn by the electrometer; and tried the strength of the shock which it gave; and found also to what distance the spark would fly. I then took two of the jars, electrified them in the same degree as before, and communicated their electricity to the two remaining. The shock of these four jars united, was.

was rather greater than that of the single jar; but the distance to which the spark would fly was only half as great.

Hence it appears, that the spark from four jars, all of the same size, will not dart to quite half so great a distance as that from one of those jars electrified in such a degree as to give a shock of equal violence; and consequently the distance to which the spark will fly is inversely in a rather greater proportion than the square root of the number of jars, supposing them to be electrified in such a degree that the shock shall be of a given strength. It must be observed, that in the last mentioned experiment, the quantity of electric fluid which passed through my body was twice as great in taking the shock of the four jars, as in taking that of the single one; but the force with which it was impelled was evidently less, and I think we may conclude, was only half as great. If so, it appears that a given quantity of electricity, impelled through our body with a given force, produces a rather less shock than twice that quantity, impelled with half that force; and consequently, the strength of the shock depends rather more on the quantity of fluid which passes through our body, than on the force with which it is impelled.

That no one could ever perceive the shock to be accompanied with any attraction or repulsion, does not seem extraordinary; for as the electricity of the torpedo is dissipated by escaping through or over the surface of its body, the instant it is produced, a pair of pith balls

suspended from any thing in contact with the animal will not have time to separate, nor will a fine thread hung near its body have time to move towards it, before the electricity is diffipated. Accordingly I have been informed by Dr. PRIESTLEY, that in discharging a battery he never could find a pair of pith balls suspended from the discharging rod to separate. But, besides, there are scarce any pith balls so fine, as to separate when suspended from a battery so weakly electrified that its shock will not pass through a chain, as is the case with that of the torpedo.

In order to examine more accurately, how far the phenomena of the torpedo would agree with electricity, I endeavoured to imitate them by means of the following apparatus. ABCFGDE fig. 3. is a piece of wood, the part ABCDE of which is cut into the shape of the torpedo, and is $16\frac{3}{4}$ inches long from A to D, and $10\frac{1}{4}$ broad from B to E; the part CFGD is 40 inches long, and serves by way of handle. MN is a glass tube let into a groove cut in the wood. ww is a piece of wire passing through the glass tube, and soldered at w to a thin piece of pewter RR lying flat on the wood, and intended to represent the upper surface of the electric organs. On the other side of the wood there is placed such another glass tube, not represented in the figure, with a wire passing through it, and soldered to another piece of pewter of the same size and shape as RR intended to represent the lower surface of those organs. The whole part ABCDE is covered with a piece of sheep's skin leather.

In making experiments with this instrument, or artificial torpedo as I shall call it, after having kept it in water of about the same saltness as that of the sea, till thoroughly soaked, I fastened the end of one of the wires, that not represented in the drawing for example, to the negative side of a large battery, and when it was sufficiently charged, touched the positive side with the end of the wire *ww*; by which means the battery was discharged through the torpedo: for as the wires were inclosed in glass tubes, which extended about an inch beyond the end of the wood *FG* no electricity could pass from the positive side of the battery to the negative, except by flowing along the wire *ww* to the pewter *rr*, and thence either through the substance of the wood, or along the wet leather, to the opposite piece of pewter, and thence along the other wire to the negative side. When I would receive a shock myself, I employed an assistant to charge the battery, and when my hands were in the proper position, to discharge it in the above mentioned manner by means of the wire *ww*. In experiments with this torpedo under water, I made use of a wooden trough; and as the strength of the shock may, perhaps, depend in some measure on the size of the trough, and on the manner in which the torpedo lies in it, I have in *fig. 4.* given a vertical section of it; the torpedo being placed in the same situation as in the figure. *ABCDE* is the trough; the length *BC* is 19 inches; the depth *AB* is 14; and the breadth is 13; consequently, as the torpedo is two inches thick in the thickest part, there is about

$5\frac{1}{2}$ inches distance between its sides and those of the trough.

The battery was composed of 49 jars, of extremely thin glass, disposed in 7 rows, and so contrived that I could use any number of rows I chose. The outsides of the jars were coated with tinfoil; but as it would have been very difficult to have coated the insides in that manner, they were filled with salt water. In a battery to answer the purpose for which this was intended, it is evidently necessary that the metals serving to make the communications between the different jars should be joined quite close; accordingly care was taken that the contacts should be made as perfect as possible. I find, by trial, that each row of the battery contains about $15\frac{3}{4}$ times as much electricity, when both are connected to the same prime conductor, as a plate of crown glass, the area of whose coating is 100 square inches, and whose thickness is $\frac{55}{1000}$ of an inch; that is, such that one square foot of it shall weigh 10 oz. 12 pwts.; and consequently, the whole battery contains about 110 times as much electricity as this plate.

The way by which this was determined, and which, I think, is one of the easiest methods of comparing the quantity of electricity which different batteries will re-

(a) I find, by experiment, that the quantity of electricity which coated glass of different shapes and sizes will receive with the same degree of electrification, is directly as the area of the coating, and inversely as the thickness of the glass; whence the proportion which the quantity of electricity in this battery bears to that in a glass or jar of any other size, may easily be computed.

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ceive with the same degree of electrification, was this: First of all, supposing a jar or battery to be electrified till the balls of the abovementioned electrometer separated to a given distance, I found how much they would separate when the quantity of electricity in that jar or battery was reduced to one-half. To do this, I took two jars, as nearly equal as possible, and electrified one of them till the balls separated to a given degree, and then communicated its electricity to the other; and observed to what distance the balls separated after this communication. It is plain, that if the jars were exactly equal, this would be the distance sought for; as in that case the quantity of electricity in the first jar would be just half as much after the communication as before; but as I could not be sure that they were exactly equal, I repeated the experiment by electrifying the second jar, communicating its electricity to the first, and observing how far the balls separated; the mean between these two distances will evidently be the degree of separation sought, though the jars were not of the same size. Having found this, I electrified one row of the battery till the balls separated to the first distance, and repeatedly communicated its electricity to the plate of coated crown glass, taking care to discharge the plate each time before the communication was made, till it appeared by the electrometer, that the quantity of electricity in that row was reduced to one half. I found it necessary to do this between 11 or 12 times, or $11\frac{1}{4}$ times as I estimate it. Whence the quantity of electric fluid in the row may be thus determined.

Let.

Let the quantity in the plate be to that in the row as x to 1; it is plain, that the electricity in the row will be diminished each time it is communicated to the plate, in the proportion of 1 to $1+x$, and consequently after being communicated $11\frac{1}{4}$ times will be reduced in the proportion

of 1 to $\overline{1+x}^{11\frac{1}{4}}$; therefore, $\overline{1+x}^{11\frac{1}{4}}=2$; and $1+x=2^{\frac{1}{11\frac{1}{4}}}$.

Whence the value of x may easily be found by logarithms. But the readiest way of computing it, and which is exact enough for the purpose, is this: multiply the number of times which you communicated the electricity of the row to the plate, by 1,444; and from the product subtract the fraction $\frac{1}{2}$; the remainder is equal to $\frac{1}{x}$, or the number of times by which the electricity in the row exceeds that in the plate.

The way by which I estimated the strength of the charge given to the battery, was taking a certain number of jars, and electrifying them till the balls of the electrometer separated to a given distance, and then communicating their electricity to the battery. This method proved very convenient; for by using always the same jars, I was sure to give always the same charge with great exactness; and by varying the number and size of the jars, I could vary the charge at pleasure, and besides could estimate pretty nearly the proportion of the different charges to each other. It was also the only convenient method which occurred to me; for I could not have done it conveniently by charging the whole battery till an electrometer suspended from it separated

parated to a given distance; because in most of the experiments the electricity was so weak, that a pair of fine pith balls suspended from the battery would separate only to a very small distance; and counting the number of revolutions of the electrical machine is a very fallacious method.

I found, upon trial, that though a shock might be procured from this artificial torpedo, while held under water, yet there was too great a disproportion between its strength, when received this way, and in air; for if I placed one hand on the upper, and the other on the lower surface of the electric organs, and gave such a charge to the battery, that the shock, when received in air, was as strong as, I believe, that of the real torpedo commonly is; it was but just perceptible when received under water. By increasing the charge, indeed, it became considerable; but then this charge would have given a much greater shock out of water than the torpedo commonly does. The water used in this experiment was of about the same degree of saltiness as that of the sea; that being the natural element of the torpedo, and what Mr. WALSH made his experiments with. It was composed of one part of common salt dissolved in 30 of water, which is the proportion of salt usually said to be contained in sea water. It appeared also, on examination, to conduct electricity not sensibly better or worse than some sea water procured from a mineral water warehouse. It is remarkable, that if I used fresh water instead of salt, the shock seemed very little weaker, when

received under water than out; which not only confirms what was before said, that salt water conducts much better than fresh; but, I think, shews, that the human body is also a much better conductor than fresh water: for otherwise the shock must have been much weaker when received under fresh water than in air.

As there appeared to be too great a disproportion between the strength of the shock in water and in air, I made another torpedo, exactly like the former, except that the part ABCDE instead of wood was made of several pieces of thick leather, such as is used for the soles of shoes, fastened one over the other, and cut into the proper shape; the pieces of pewter being fixed on the surface of this, as they were on the wood, and the whole covered with sheep skin like the other. As the leather, when thoroughly soaked with salt water, would suffer the electricity to pass through it very freely, I was in hopes that I should find less difference between the strength of the shock in water and out of it, with this than with the other. For suppose that in receiving the shock of the former torpedo under water, the quantity of electricity which passed through the wood and leather of the torpedo, through my body, and through the water, were to each other as T, B, and W; the quantity of electricity, which would pass through my body, when the shock was received under water, would be to that which would pass through it, when the shock was received out of water, as $\frac{B}{B+T+W}$ to $\frac{B}{B+T}$; as in the first case, the quantity

which would pass through my body would be the

$\frac{B}{B+T+W}$ part of the whole; and in the latter the $\frac{B}{B+T}$ part.

Suppose now, that the latter torpedo conducts N times better than the former; and consequently, that in receiving its shock under water, the quantity of electricity which passes through the torpedo, through my body, and through the water, are to each other as NT , B , and w ; the quantity of electricity which will now pass through my body, when the shock is received under water, and

out of water, will be to each other as $\frac{B}{B+NT+W}$ to $\frac{B}{B+NT}$;

which two quantities differ from each other in a less pro-

portion than $\frac{B}{B+T+W}$ and $\frac{B}{B+T}$: consequently, the readier

the body of the torpedo conducts, the greater charge will it require to give the same shock, either in water or out of it; but the less will be the difference between the strength of the two shocks. It should be observed, that this alteration, so far from making it less resembling the real torpedo, in all probability makes it more so; for I see no reason to think, that the real torpedo is a worse conductor of electricity than other animal bodies; and the human body is at least as good, if not a much better conductor than this new torpedo.

The event answered my expectation; for it required about three times as great a charge of the battery, to give the same shock in air, with this new torpedo as with the former; and the difference between its strength when

received under water and out of it, was much less than before, and perhaps not greater than in the real torpedo. There is, however, a considerable difference between the feel of it under water and in air. In air it is felt chiefly in the elbows; whereas, under water, it is felt chiefly in the hands, and the sensation is sharper and more disagreeable. The same kind of shock, only weaker, was felt if, instead of touching the sides, I held my hands under water at two or three inches distance from it.

It is remarkable, that I felt a shock of the same kind, and nearly of the same strength, if I touched the torpedo under water with only one hand, as with both. Some gentlemen who repeated the experiment with me thought it was rather stronger. This shews, that the shock under water is produced chiefly by the electricity running through one's hand from one part to the other; and that but a small part passes through one's body from one hand to the other. The truth of this will appear with more certainty from the following circumstance; namely, that if I held a piece of metal, a large spoon for instance, in each hand, and touched the torpedo with them instead of my hands, it gave me not the least shock when immersed in water; though when held in air, it affected me as strongly if I touched it with the spoons as with my hands. On increasing the charge, indeed, its effect became sensible, and as well as I could judge, the battery required to be charged about twelve times as high to give the same shock when the torpedo was touched with the spoons under water as out of it. It must be observed, that in trying this

this experiment, as my hands were out of water, I could be affected only by that part of the fluid which passed through my body from one hand to the other.

The following experiments were made with the torpedo in air. If I stood on an electric stool, and touched either surface of the electric organs with one hand only, I felt a shock in that hand; but scarcely so strong as when touching it in the same manner under water. If I laid a hand on one surface of the electric organs, and with the other touched the tail, I felt a shock; but much weaker than when touching it in the usual manner; that is, with one hand on the upper surface of those organs, and the other on the lower. If I laid a thumb on either surface of an electric organ, and a finger of the same hand on any part of the body, except on or very near the same surface of the organs, I felt a small shock.

In all the foregoing experiments, the battery was charged to the same degree, except where the contrary is expressed: they all seem to agree very well with Mr. WALSH's experiments.

Mr. WALSH found, that if he inclosed a torpedo in a flat basket, open at the top, and immersed it in water to the depth of three inches, and while the animal was in that situation, touched its upper surface with an iron bolt held in one hand, while the other hand was dipped into the water at some distance, he felt a shock in both of them. I accordingly tried the same experiment with the artificial torpedo; and if the battery was charged about six times as high as usual, received a small shock in each hand.

hand (*a*). No sensible difference could be perceived in the strength, whether the torpedo was inclosed in the basket or not. The trough in which this experiment was tried was 36 inches long, $14\frac{1}{2}$ broad, and 16 deep; and the distance of that hand which was immerfed in the water from the electric organs of the torpedo, was about 14 inches. As it was found necessary to charge the battery so much higher than usual, in order to receive a shock, it follows, that unless the fish with which Mr. WALSH tried this experiment were remarkably vigorous, there is still too great a disproportion between the strength of the shock of the artificial torpedo when received under water and out of it. If this is the case, the fault might evidently be remedied by making it of some substance which conducts electricity better than leather.

When the torpedo happens to be left on shore by the retreat of the tide, it loosens the sands by flapping its fins, till its whole body, except the spiracles, is buried; and it is said to happen sometimes, that a person accidentally treading on it in that situation, with naked feet, is thrown down by it. I therefore filled a box, 32 inches long and 22 broad, with sand, thoroughly soaked with salt water, to the depth of four inches, and placed the torpedo in it, intirely covered with the sand, except the upper part of its convex surface, and laid one hand on its electrical organs, and the other on the wet sand about 16 inches from

(*a*) As well as I could judge, the battery required to be charged about 16 or 20 times as high, to give a shock of the same strength when received this way as when received in the usual manner with the torpedo out of water.

it.

it. I felt a shock, but rather weak; and as well as I could judge, as strong as if the battery had been charged half as high, and the shock received in the usual way.

I next took two thick pieces of that sort of leather which is used for the soles of shoes, about the size of the palm of my hand; and having previously prepared them by steeping in salt water for a week, and then pressing out as much of the water as would drain off easily, repeated the experiment with these leathers placed under my hands. The shock was weaker than before, and about as strong as if received in the usual way with the battery charged one-third part as high. As it would have been troublesome to have trod on the torpedo and sand, I chose this way of trying the experiment. The pieces of leather were intended to represent shoes, and in all probability the shoes of persons who walk much on the wet sand will conduct electricity as well as these leathers. I think it likely, therefore, that a person treading in this manner on a torpedo, even with shoes on, but more so without, may be thrown down, without any extraordinary exertion of the animal's force, considering how much the effect of the shock would be aided by the surprise.

One of the fishermen that Mr. WALSH employed assured him, that he always knew when he had a torpedo in his net, by the shocks he received while the fish was at several feet distance; in particular, he said, that in drawing in his nets with one of the largest in them, he received a shock when the fish was at twelve feet

feet distance, and two or three more before he got it into his boat. His boat was afloat in the water, and he drew in the nets with both hands. It is likely, that the fisherman might magnify the distance; but, I think, he may so far be believed, as that he felt the shock before the torpedo was drawn out of water. This is the most extraordinary instance I know of the power of the torpedo; but I think seems not incompatible with the supposition of its being owing to electricity; for there can be little doubt, but that some electricity would pass through the net to the man's hands, and from thence through his body and the bottom of the boat, which in all probability was thoroughly soaked with water, and perhaps leaky, to the water under the boat: the quantity of electric fluid, however, taking this circuit, would most likely bear so small a proportion to the whole, that this effect can not be accounted for, without supposing the fish to exert at that time a surprizingly greater force than what it usually does.

Hitherto, I think, the effects of this artificial torpedo agree very well with those of the natural one. I now proceed to consider the circumstance of the shock's not being able to pass through any sensible space or air. In all my experiments on this head, I used the first torpedo, or that made of wood; for as it is not necessary to charge the battery more than one-third part as high to give the same shock with this as with the other, the experiments were more likely to succeed, and the conclusions to be drawn from them would be scarcely less convincing: for
I find

I find, that five or six rows of my battery will give as great a shock with the leathern torpedo, as one row electrified to the same degree will with the wooden one; consequently, if with the wooden torpedo and my whole battery, I can give a shock of a sufficient strength, which yet will not pass through a chain of a given number of links, there can be no doubt, but that, if my battery was five or six times as large, I should be able to do the same thing with the leathern torpedo.

I covered a piece of sealing wax on one side with a slip of tinfoil, and holding it in one hand, touched an electrical organ of the torpedo with the end of it, while my other hand was applied to the opposite surface of the same organ. The shock passed freely, being conducted by the tinfoil; but if I made, with a penknife, as small a separation in the tinfoil as possible, so as to be sure that it was actually separated, the shock would not pass, conformably to what Mr. WALSH observed of the torpedo.

I tried the experiment in the same manner with the LANE's electrometer described in p. 202, and found that the shock would not pass, unless the knobs were brought so near together as to require the assistance of a magnifying glass to be sure that they did not touch.

I took a chain of small brass wire, and holding it in one hand, let the lowest link lie on the upper surface of an electric organ, while my other hand was applied to the opposite surface. The event was, that if the link, held in my hand, was the fifth or sixth from the bottom, and consequently, that the electricity had only four or five links

to pass through besides that in my hand, I received a shock; so that the electricity was able to force its way through four or five intervals of the links, but not more. One gentleman, indeed, found it not to pass through a single interval; but in all probability the link which lay on the torpedo happened to bear more loosely than usual against that in his hand. If instead of this chain I used one composed of thicker wire, the shock would pass through a great number of links; but I did not count how many. It must be observed, that the principal resistance to the passage of the electrical fluid is formed by the intervals of the lower links of the chain; for as the upper are stretched by a greater weight, and therefore pressed closer together, they make less resistance. Consequently the force required to make the shock pass through any number of intervals, is not twice as great as would be necessary to make it pass through half the number. For the same reason it passes easier through a chain consisting of heavy links than of light ones.

Whenever the electricity passed through the chain, a small light was visible, provided the room was quite dark. This, however, affords no argument for supposing that the phenomena of the torpedo are not owing to electricity; for its shock has never been known to pass through a chain or any other interruption in the circuit; and consequently, it is impossible that any light should have been seen.

In all these experiments, the battery was charged to the same degree; namely, such that the shock was nearly
of

of the same strength as that of the leathern torpedo, and which I am inclined to think, from my conversation with Mr. WALSH, may be considered as about the medium strength of those of a real one of the same size as this. It was nearly equal to that of the plate of crown glass in p. 206. electrified to such a degree as to discharge itself when the knobs of a LANE's electrometer were at .0115 inches distance; whence a person, used to electrical experiments, may ascertain its strength. The way I tried it was by holding the LANE's electrometer in one hand, with the end resting on the upper surface of the plate, and touching the lower surface with the other hand, while an assistant charged the plate by its upper side till it discharged itself through the electrometer and my body. There is, however, a very sensible difference between the sensation excited by a small jar or plate of glass like this, and by a large battery electrified so weakly that the shock shall be of the same strength; the former being sharper and more disagreeable. Mr. WALSH took notice of this difference; and said, that the artificial torpedo produced just the same sensation as the real one.

As it appeared, that a shock of this strength would pass through a few intervals of the links of the chain, I tried what a smaller would do. If the battery was charged only to a fourth or fifth part of its usual height, the shock would not pass through a single interval; but then it was very weak, even when received through a piece of brass wire, without any link in it. This chain was quite clean and very little tarnished; the lowest link was larger

than the rest, and weighed about eight grains. If I used a chain of the same kind, the wire of which, though pretty clean, was grown brown by being exposed to the air, the shock would not pass through a single interval, with the battery charged to about one-third or one-half its usual strength.

It appears, that in this respect the artificial torpedo does not completely imitate the effects of the real one, though it approaches near to it; for the shock of the former, when not stronger than that of the latter frequently is, will pass through four or five intervals of the links of a chain; whereas the real torpedo was never known to force his through a single interval. But, I think, this by no means shews, that the phenomena of the torpedo are not produced by electricity; but only that the battery I used is not large enough. For we may safely conclude, from the experiments mentioned in p. 200. and 202. that the greater the battery is, the less space of air, or the fewer links of a chain, will a shock of a given strength pass across. For greater certainty, however, I tried, whether if the whole battery and a single row of it were successively charged to such a degree, that the shock of each should be of the same strength when received through the torpedo in the usual manner, that of the whole battery would be unable to pass through so many links of a chain as that of a single row^(b). In order to which I made the following machine.

(b) The battery, as was before said, was divided into seven rows, each of which could be used separately.

GM, fig. 5. is a piece of dry wood; Ff, Ee, Dd, Cc, Bb, and Aa, are pieces of brass wire fastened to 1., and turned up at bottom into the form of a hook, on which is hung a small brass chain, as in the figure, so as to form five loops, each loop consisting of five links; the part G is covered with tinfoil, which is made to communicate with the wire Aa. If I held this piece of wood in one hand, with my thumb on either of the wires Ff, Ee, &c. and applied the part G to one surface of an electric organ, while with a spoon, held in the other hand, I touched the opposite surface, I received a shock, provided the battery was charged high enough, the electricity passing through all that part of the chain between Aa, and my thumb; so that I could make the shock pass through more or fewer loops, according to which wire my thumb was placed on; but if the charge was too weak to force a passage through the chain, I felt no shock, as the wood was too dry to convey any sensible quantity of electricity. The event of the experiment was, that if I charged the whole battery to such a degree that the shock would but just pass through two loops of the machine, and then charged a single row to such a degree as appeared, on trial, just sufficient to give a shock of the same strength as the former, it passed through all five loops; whether it would have passed through more I cannot tell. If, on the other hand, I gave such a charge to the whole battery, and also to the single row, as was just sufficient to force a passage through two loops of the chain, the shock with the whole battery was much stronger than that with the single row.

It must be observed, that in the foregoing machine, each loop consisted of the same number of links, and the links of each loop were stretched by the same weight; so that it required no more force to impell the electricity through one loop than another, which was my reason for using this machine rather than a plain chain. Considerable irregularities occurred in trying the above experiments, and indeed all those with a chain; for it frequently happened, that the shock would not pass with the battery charged to a certain degree, when perhaps a minute after, it would pass with not more than three-fourths of the charge. The irregularity, however, was not so great but that, I think, I may be certain of the truth of the foregoing facts; especially as the experiments were repeated several times. The uncertainty was at least as great in the experiments with LANE's electrometer, when the knobs were brought so close together, as is necessary in experiments of this kind.

It appears therefore, that if the whole battery, and a single row of it, are both charged in such a degree as to give a shock of the same strength, the shock with the whole battery will pass through fewer loops of the chain than that with the single row; so that, I think, there can be no doubt, but that if the battery had been large enough, I should have been able to give a shock of the usual strength, which yet would not have passed through a single interval of the links of a chain.

On the whole, I think, there seems nothing in the phenomena of the torpedo at all incompatible with electricity;

Fig. 4.

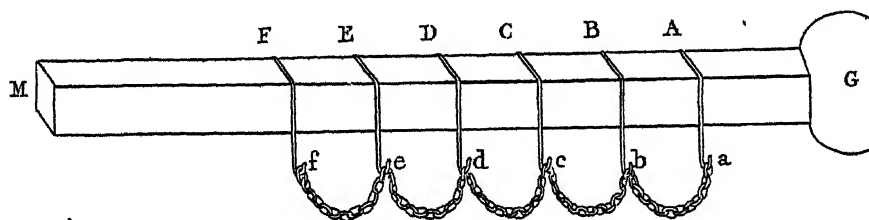
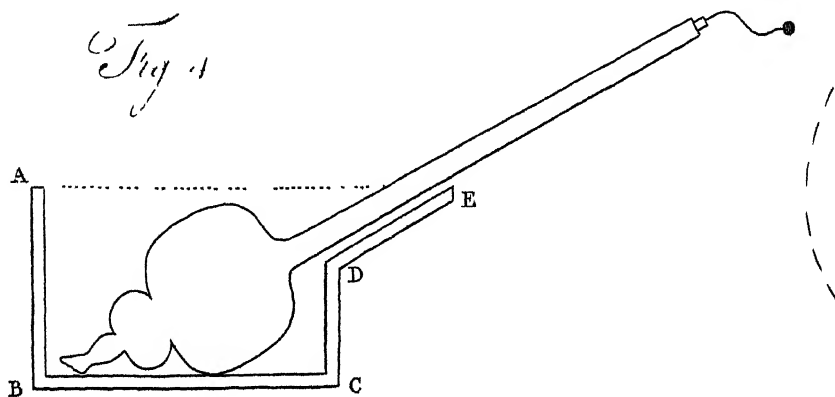
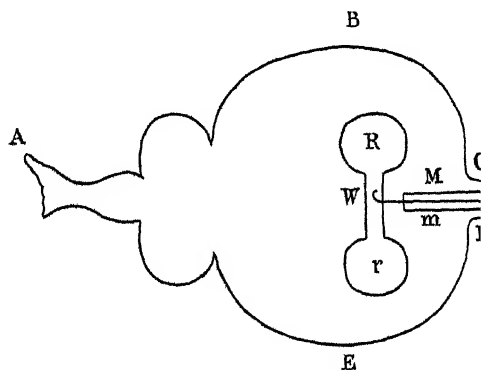
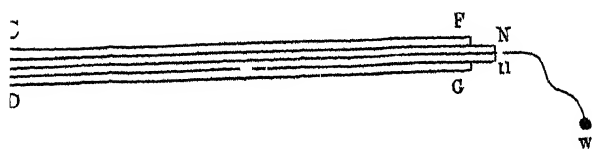
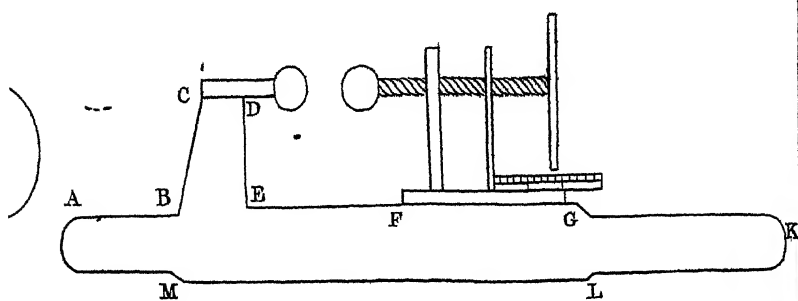
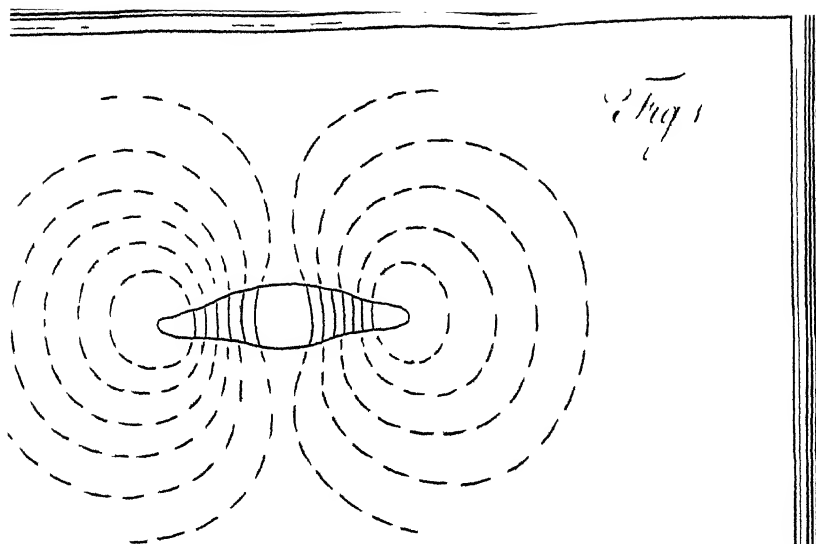


Fig. 5.





tricity; but to make a compleat imitation of them, would require a battery much larger than mine. It may be asked, where can such a battery be placed within the torpedo? I answer, perhaps it is not necessary that there should be any thing analogous to a battery within it. The case is this; it appears, that the quantity of electric fluid, transferred from one side of the torpedo to the other, must be extremely great; for otherwise it could not give a shock, considering that the force with which it is impelled is so small as not to make it pass through any sensible space of air. Now if such a quantity of fluid was to be transferred at once from one side to the other, the force with which it would endeavour to escape would be extremely great, and sufficient to make it dart through the air to a great distance, unless there was something within it analogous to a very large battery. But if we suppose, that the fluid is gradually transferred through the electrical organs, from one side to the other, at the same time that it is returning back over the surface, and through the substance, of the rest of the body; so that the quantity of fluid on either side is during the whole time very little greater or less than what is naturally contained in it; then it is possible, that a very great quantity of fluid may be transferred from one side to the other, and yet the force with which it is impelled be not sufficient to force it through a single interval of the links of a chain. There seems, however, to be room in the fish for a battery of a sufficient size; for Mr. HUNTER has shewn, that each of the prismatical columns of
which

which the electrical organ is composed, is divided into a great number of partitions by fine membranes, the thickness of each partition being about the 150th part of an inch; but the thickness of the membranes which form them is, as he informs me, much less. The bulk of the two organs together in a fish 10 $\frac{1}{3}$ inches broad, that is of the same size as the artificial torpedos, seems to be about 24 $\frac{1}{2}$ cubic inches; and therefore the sum of the areas of all the partitions is about 3700 square inches. Now 3700 square inches of coated glass $\frac{1}{150}$ of an inch thick will receive as much electricity as 30500 square inches, 055 of an inch thick^(a); that is, 305 times as much as as the plate of crown glass mentioned in p. 206, or about 2 $\frac{3}{4}$ times as much as my battery, supposing both to be electrified by the same conductor; and if the glass is five times as thin, which perhaps is not thinner than the membranes which form the partitions, it will contain five times as much electricity, or near fourteen times as my battery.

It was found, both by Dr. WILLIAMSON and by a committee appointed by the Philosophical Society of Pennsylvania, that the flock of the *Gymnotus* would sometimes pass through a chain, though they never perceived any light. I therefore took the same chain which I used in the foregoing experiments, consisting of 25 links, and suspended it by its extremities from the extreme hooks of the machine described in p. 221, and applying the end of the machine to the negative side of the battery,

^(a) Vide Note in p. 206.

touched the positive side with a piece of metal held in the other hand, so as to receive the shock through the chain without its passing through the torpedo; the battery being charged to such a degree that the shock was considerably stronger than what I usually felt in the foregoing experiments. I found that if the chain was not stretched by any additional weight, the shock did not pass at all: If it was stretched by hanging a weight of seven pennyweights to the middle link, it passed, and a light was visible between some of the links; but if fourteen pennyweights were hung on, the shock passed without my being able to perceive the least light, though the room was quite dark; the experiment being tried at night, and the candle removed before the battery was discharged. It appears, therefore, that if in the experiments made by these gentlemen the shock never passed, except when the chain was somewhat tense, which in all probability was the case, the circumstance of their not having perceived any light is by no means repugnant to the supposition that the shock is produced by electricity.

XIII. *Observations on Respiration, and the Use of the Blood.*
 By Joseph Priestley, LL.D. F. R. S.

R. Jan. 25, 1776. **T**HERE is, perhaps, no subject in physiology, and very few in philosophy in general, that has engaged more attention than that of the use of *respiration*. It is evident, that without breathing most animals would presently die; and it is also well known, that the same air will not long answer the purpose: for if it has been frequently respired, the breathing of it is as fatal as the total deprivation of air. But by what property it is, that air contributes to the support of animal life; and why air that has been much breathed will no more answer the purpose, seems not to have been discovered by any of the many philosophers and physicians who have professedly written upon the subject; and it might have continued to elude all *direct investigation*, when it discovered itself, without any trouble or thought, in the course of my researches into the properties of different kinds of air, which had at first required another object.

In these experiments it clearly appeared, that respiration is a *phlogistic process*, affecting air in the very same manner

as every other phlogistic process (*viz.* putrefaction, the effervescence of iron-filings and brimstone, or the calcination of metals, &c.) affects it; diminishing the quantity of it in a certain proportion, lessening its specific gravity, and rendering it unfit for respiration or inflammation, but leaving it in a state capable of being restored to a tolerable degree of purity by agitation in water, &c. Having discovered this, I concluded, as may be seen Phil. Trans. vol. LXII. p. 187. and *Observations upon Air*, vol. I. p. 78. 277, that the use of the lungs is to carry off a putrid *effluvium*, or to discharge that phlogiston, which had been taken into the system with the aliment, and was become, as it were, *effete*; the air that is respired serving as a *menstruum* for that purpose.

What I then concluded to be the use of *respiration* in general, I have now, I think, proved to be effected by means of the *blood*, in consequence of its coming so nearly into contact with the air in the lungs; the blood appearing to be a fluid wonderfully formed to imbibe, and part with, that principle which the chemists call phlogiston, and changing its colour in consequence of being charged with it, ~~or being freed from it~~; and affecting air in the very same manner, ~~both out of the body and in the lungs~~; and even notwithstanding the interposition of various substances, which prevent its coming into immediate contact with the air.

As it may not be unpleasing or unuseful, I shall, before I relate my own experiments, briefly recite the principal of the opinions which have been held con-

cerning the use of respiration, from HALLER's excellent *System of Physiology*, and some others of the most eminent writers upon that subject.

HIPPOCRATES reckoned air among the *aliments* of the body. But it was more generally the opinion of the ancients, that, there being a kind of *vital fire* kept up in the heart, the heat of the blood was tempered in the lungs. GALEN also supposed, that there was something equivalent to a fire constantly kept up in the heart; and that the chief use of the lungs was to carry off such vapours as were equivalent to smoke thrown off from that fire. HALLER, vol. III. p. 354. CARTESIUS maintained the same vital fire in the heart, supposing that air was necessary for cooling and condensing the blood. *Ibid.* p. 343.

Of the more modern physiologists, some have thought that the air itself is taken into the lungs; others, that it is only something extracted from the air, as the more subtle parts of that fluid, an ether, or aerial nitre; while others suppose it to be the air itself, but dissolved in water, and therefore in an unelastic state, *ibid.* p. 321.

Most of those who think that air is taken into the blood suppose it to be taken in by the lungs, *ibid.* p. 330. Some suppose, that the effect of the admission of this air into the blood is a fermentation, p. 332. Others suppose, that it acts by its spring, preventing the too close contact of the globules, and thereby preserving its fluidity, intestine motion, and heat, *ibid.* BERTIER supposed, that the circulation of the blood was, in a great measure, owing to the admission of air into it. VAN HELMONT ascribed the volatility of the fixed elements

ments in the food to this air, p. 336.; and STEVENSON thought, that the air which had circulated in the blood, and which had heated the blood too much, was exhaled by the lungs, p. 355.

Others say, that the air itself is not admitted into the blood, but only some active, spirituous, and ethereal particles; that this vital spirit passes from the lungs to the heart and arteries, and at length becomes the animal spirits, which are by this means generated from the air, p. 333. Others, who do not admit that the animal spirits are derived from the air, still say that some other *vital principle* comes from thence. This vital principle MALPIGHIIUS supposes to be a saline vapour; LISTER, a hot, inflammable, sulphureous spirit; VIEUSSENIUS, a volatile acid salt, which keeps up the fermentation of the blood; and BRYAN ROBINSON, the aerial acid, which preserves the blood from putrefaction; preserves also its density, and strengthens the animal fibres. For this reason he supposes it is that we feel ourselves refreshed in cold air, as it abounds with a more plentiful acid quality, p. 334. They who suppose that nitre is taken from the air into the blood, ascribe to that principle its fermentation, its heat, and its density, p. 334.

It is a received opinion, that one use of the lungs is to attenuate the blood, p. 359; and MALPIGHIIUS adds, that by this means, the different particles of the blood become thoroughly mixed together; while others think that the blood is condensed in the lungs; and others, that the globules, and all the finer humours, receive their configuration there, *ibid.* Some, without considering the
air

air as of any other use than to put the lungs in motion, think, that heat is produced in the lungs by the attrition of the blood in passing through them. *Misc. Taurin.* vol. V. p. 36. The red colour of the blood has been thought by some to be caused by this attrition in the lungs; but LOWER refuted this notion, chiefly by observing, that the attrition of the blood is greater in the muscles, from which, however, it always returns black, *Ibid.* vol. I. p. 74.

Dr. WHYTT thought there was something of a vital and stimulating nature derived from the air into the blood, by means of which it made the heart to contract, HALLER, vol. III. p. 336.

BOERHAAVE says, that air not changed is deadly; not on account of heat, rarefaction, or density, but for some other occult cause. *Misc. Taurin.* vol. V. p. 30.

Dr. HALES, who has thrown much more light upon the doctrine of air than all his predecessors, was equally ignorant of the use of it in respiration; and at different times seems to have adopted different opinions concerning it.

In his *Statical Essays*, vol. II. p. 321. he supposes, that air is rendered alkaline by breathing, and corrected, in some measure, by the fumes of vinegar.

In agreement, as he observes, with BOERHAAVE, he says, p. 100. that the blood acquires its warmth chiefly in the lungs, where it moves with much greater rapidity than in any other capillary vessels of the body, vol. II. p. 87; but that one use of the air is to cool the blood, p. 94; and he makes an estimate of the degree of this refrigeration. The red colour of the globules of blood, he says, p. 88, intimates

intimates their abounding with sulphur, which makes them more susceptible and retentive of heat than those bodies which have less of it.

He also supposes, p. 102, that another great use of the lungs is to attenuate and separate the globules of blood; and that the floridness of the arterial blood above the venal may, in a good measure, be owing to the strong agitation, friction, and comminution, which it undergoes in passing through them. In like manner, in an experiment which he made for the purpose, blood much agitated in a close glass vessel was observed to be very florid, not only on its surface, but through its whole substance, as arterial blood is, vol. II. p. 102. I would observe, however, that in this experiment, the blood must have acquired its florid colour from the air with which it was agitated.

He adds, that it is probable, that the blood may, in the lungs, receive some other important influence from the air, which is in such great quantities inspired into them. In other places, however, he explodes the doctrine of a *vivifying spirit* in the air. It has long, he says, been the subject of inquiry to many, to find of what use it is in respiration; which, though it may in some respects be known, yet it must be confessed, that we are still much in the dark about it, vol. II. p. 102.

Suffocation, he says, vol. II. p. 271, consists chiefly in the falling flat of the lungs, occasioned by the grossness of the particles of a thick noxious air, they being, in that floating state, most easily attracted by each other, as we find that sulphur, and the elastic repelling particles of air are;

and consequently unelastic, fulphureous, saline, and other floating particles, will most easily coalesce, whereby they are rendered too gross to enter the minute vesicles, which are also much contracted, as well by the loss of the elasticity of the confined air, as by the contraction occasioned by the stimulating acid fulphureous vapours. And hence it is not improbable, that one great design of nature in the structure of this important and wonderful *viscus*, was to frame the vesicles so very minute, thereby effectually to hinder the ingress of gross, feculent particles, which might be injurious to the animal economy.

Lastly, he concludes, that the effect of respiration is to abate, and in part destroy, the elasticity of the air; and as this was effected by sulphureous vapours, and he could breathe for a longer time air that had passed through cloaths dipped in a solution of salt of tartar, he concluded, that the air had been mended by the tartar having strongly imbibed the sulphureous, acid, and watery vapours, vol. I. p. 267.

HALLER, after reciting the opinions of all that had gone before him, supposes, with Dr. HALES, that, in consequence of the air losing its spring in the lungs, they cannot be kept dilated; and therefore, they must collapse, and the circulation of the blood be impeded, vol. III. p. 258. When he states his opinion concerning the use of the lungs more fully, he says, that the true use of them is partly inhaling, and partly exhaling, p. 351. 'That the lungs inhale both water and air; but that in the lungs the air loses its elastic property, so as to be easily soluble in water or vapour, p. 352.: and he thinks it probable,

probable, that this air serves as a cement to bind the earthy parts together. He also makes no doubt, but that various other matters, miscible with water, are inhaled by the lungs; and he even thinks it not improbable, that the air may carry some electric virtue along with it. The principal exhalation of the lungs, he thinks, to be water, abounding with oily, volatile, and saline principles; and these oily and fetid vapours, he thinks, to be the *fuligines* of GALEN and other ancients, p. 354.

Mr. CIGNA of Turin, has given much attention to this curious subject, as appears by two Memoirs of his; one in the first volume of the *Miscellanea Taurinensia*, in which he very well accounts for the florid red colour of the blood; and the other, which is a much more elaborate Memoir, intitled, *De Respiratione*, in the fifth volume of the same work, just published, or about to be published, the copy of the article having been sent to me by the author.

He takes it for granted, that air which has once been breathed is unfit for farther respiration, on no other account than its being loaded with *noxious vapours*, which discover themselves by a fetid smell. *Misc. Taurin.* vol. V. p. 30. And he takes it for granted, that the elasticity of air is diminished by respiration, though he does not consider that diminution of elasticity as the cause of its noxious quality. He therefore concludes, that air which has been breathed, suffocates by means of the irritation which it occasions to the lungs, by which the bronchia, and the lungs themselves, are contracted, so as to resist the entrance of the air; and therefore, that respired air is

noxious on the same account as mephitic vapours, or those of burning brimstone, p. 31; that, in frequently breathing the same air, it becomes so loaded with these vapours, as to excite a convulsion in the lungs, and thereby render them unfit for transmitting the blood, p. 42.

This philosopher supposes that air enters the pores of the blood, retaining its elastic power, p. 50. and that it continues at rest there, because its endeavour to escape is counteracted by the equal pressure of the ambient medium, p. 52. This air, he supposes to be introduced into the blood by the chyle, and never by the way of the lungs, except when, by some means or other, the equilibrium between the air in the blood and the external air is lost, p. 57. If the external air be rarer than the internal, the air in the blood, expanding itself, will inflate the animal, and have the same effect as air introduced into the veins.

What we are chiefly indebted to M. CIGNA for, is his decisive experiments with respect to the florid colour of the blood, which he clearly proves to be caused by the contact of air; though he afterwards seems willing to desert that hypothesis. It was often imagined, that the reason why the lower part of a quantity of blood was black, while the surface was red, was, that the black particles, being heavier than the rest, subsided to the bottom; but this opinion our author clearly refutes. He found, that when he put a little oil upon a quantity of blood, it remained black throughout; but that when he took away the red part, and exposed to the air the lower
laminae,

laminæ, which were black, they also became successively red, till the whole mass acquired that colour, *Misc. Taurin.* vol. I. p. 73. Also, at the request of M. CIGNA, Father BECCARIA tried what would be the effect of exposing blood in *vacuo*; and he found, that in those circumstances, it always continued black; but that, by exposing it again to the air, it became red, p. 68.

M. CIGNA concludes his first dissertation with observing, that it is not easy to say how it comes to pass, that the lower part of a mass of blood becomes black, whether by the air which it had imbibed escaping from it, or by its depositing something saline, necessary to contribute to its redness, or by the pressure of the atmosphere; but he inclines to think, that air mixed with blood, and interposed between the globules, preserves its redness: but that by concreting it is expelled from it, or becomes so fixed as to be incapable of making it red. This opinion, he thinks, is rendered in some measure probable, by the increased density of concremented blood, and by the emission of air from other fluids in a concrement state, p. 74.

Notwithstanding what he had advanced in his first Memoir, yet in the second, which was written several years after it, he doubts whether the change of colour in the blood takes place in the lungs; but if it does, he inclines to ascribe this effect to the *evaporation* from the blood in the lungs: and though he always found, that the colour of the blood was changed by the contact of air, yet when he considered that evaporation must, as he thought, necessarily attend the contact of air, he imagined, that this

effect might equally be attributed to this circumstance. But he acknowledges, that this hypothesis ought not to be received till it be confirmed by experiments, *Misc. Taurin.* vol. V. p. 61.

Upon the whole, he concludes, that the principal use of air to the *blood*, is to preserve the equilibrium with the external air, and to prevent the vessels from being rendered unfit to transmit the blood, on account of the external pressure; whereas, by means of the air they contain, the fluids move in their proper vessels as freely as in *vacuo*, and the membranes and viscera also easily slide over each other, p. 63. And with respect to the use of the *lungs*, since he imagined that air is not introduced into the blood by means of them, he thinks, that because such lungs as those of man are given to the warmer animals only, the chief use of respiration is exhalation, and consequently the cooling of the blood, p. 65.

The last writer whom I shall quote upon this subject, is the late ingenious Mr. HEWSON; who says, in his *Experimental Inquiry into the Properties of Blood*, p. 9. "As the colour of the blood is changed by air out of the body, it is presumed, that the air in the lungs is the immediate cause of the same change in the body." That this change is really produced in the lungs, he is persuaded, he says, from experiments, in which he distinctly saw the blood of a more florid red in the left auricle of the heart than it was in the right; but how this effect is produced, he says, is not yet determined.

Since

Since some of the neutral salts, and particularly nitre, has a similar effect on the colour of the blood; some, says he, attribute this difference to the nitre absorbed from the air, while in the lungs. But this, he adds, is a mere hypothesis, for air contains no nitre, and most of the neutral salts produce the same effect in some degree.

Since, however, a solution of nitre does produce this effect upon blood, instantly making the very blackest of it of a beautiful florid red, though this effect is not peculiar to nitre (for a solution of common salt does nearly the same thing) I own I am inclined to ascribe this effect to the air; especially since I have proved, as I apprehend, that atmospherical air consists of earth and spirit of nitre. Possibly, therefore, the air we breathe may be so far decomposed, as to communicate something of nitre to the blood, in its passage through the lungs.

After this review of the observations and opinions of others on this important question in physiology, I shall proceed to recite my own. It may appear something extraordinary, that among such a variety of opinions concerning the use of respiration, the right one should never have been so much as conjectured, though unsupported by the proper proof. But indeed, this animal function, and the phlogistic processes in chemistry, especially that of the calcination of metals, which is, perhaps, the most simple of them, are to appearance very different things; and therefore, it is the less to be wondered, that no person should

should have imagined, they would produce the same effect on the air in which they were performed.

That respiration, however, is, in reality, a true phlogistic process, cannot, I think, admit of a doubt, after its being found, that the air which has served for this purpose is left in precisely the same state as that which has been exposed to any other phlogistic process. And since all the blood in the body passes through the lungs, and, according to Mr. HEWSON's observations and others, the remarkable change between the colour of the venal and arterial blood takes place there, it can hardly be doubted, that it is by means of the *blood* that the air becomes phlogisticated in passing through the lungs; and therefore, that one great use of the blood must be to discharge the phlogiston with which the animal system abounds, imbibing it in the course of its circulation, and imparting it to the air, with which it is nearly brought into contact, in the lungs; the air thus acting as the great menstruum for this purpose.

Though I had no doubt concerning this conclusion from my former experiments, I thought so great a problem deserved as much illustration as could be given to it; and therefore I was willing to try, whether the blood was of such a nature, as to retain any of this power of affecting air when congealed, and out of the body, that it has when it is fluid, and in the body; and the experiments have fully answered my expectations.

Having taken the blood of a sheep, and let it stand till it was coagulated, and the serum was separated from it (after which the surface, being exposed to the common

air, is well known to assume a florid red colour, while the inside is of a much darker red, bordering upon black) I introduced pieces of the crassamentum, contained in nets of open gauze, or of wire, sometimes through water, and sometimes through quicksilver, into different kinds of air, and always found that the blackest parts assumed a florid red colour in common air, and more especially in dephlogisticated air, which is purer and more fit for respiration than common air (and accordingly the blood always acquired a more florid colour, and the change was produced in less time in this than in common air) whereas the brightest red blood became presently black in any kind of air that was unfit for respiration, as in fixed air, inflammable air, nitrous air, or phlogisticated air; and after becoming black in the last of these kinds of air, it regained its red colour upon being again exposed to common air, or dephlogisticated air; the same pieces becoming alternately black and red, by being transferred from phlogisticated to dephlogisticated air; and *vice versa*.

In these experiments the blood must have parted with its phlogiston to the common air, or dephlogisticated air, and have imbibed it, and have become saturated with it, when exposed to phlogisticated, nitrous, inflammable, or fixed air. The only difficulty is with respect to the fixed air; for all the other kinds certainly contain phlogiston. But, as I have observed in the account of my experiments on vitriolic acid air, phlogiston seems to be necessary to the constitution of every kind of air; and besides, the blackness of the blood may arise from other causes than:

than its acquiring phlogiston. GABER, for instance, observes, that blood becomes black when it begins to putrify, as it does also whenever it is dried and hardened near the fire. Father BECCARIA also found, as I have observed, that red blood *continued* (and he could hardly fail to observe also, that it *became*) black in *vacuo*, where it could not have imbibed phlogiston. This I found to be the case when the blood was covered two inches and a half with ferum; but it regained its florid colour when it was exposed to the open air.

In general, however, it cannot be expected, that when blood has become black without having received phlogiston *ab extra*, it will recover its florid colour by being exposed to the air. For the delicacy of its texture, and consequently its capacity of being easily affected by phlogiston, may be essentially altered by internal causes of blackness. This is even the case when blood has become black by being exposed to nitrous and inflammable air, though this change is probably effected by its imbibing phlogiston.

I exposed pieces of the same mass of red blood to these two kinds of air, and also to fixed air at the same time. They all became black; but that which was in the inflammable air was the least so, and none of them recovered their florid colour in the open air. But at another time, a piece of crassamentum, which had become black in fixed air, did, in some measure, and very slowly, recover its florid colour in dephlogisticated air. Perhaps the pieces that had lost their colour in the nitrous and inflam-

inflammable air might have recovered it by means of this more powerful menstruum.

Since, however, blood, after becoming black in phlogisticated air, is always capable of resuming its red colour on being again exposed to pure air, it may be concluded, that the preceding blackness, discharged in the pure air, and producing the constant effect of phlogiston, in depraving the air, was owing to the phlogiston it had imbibed in the former situation, and which it parted with in the latter. And this is remarkably the case when blood is transferred from phlogisticated into dephlogisticated air. Even the circumstance of the *deeper colour* is sufficient to give a chemist a suspicion that it contains more phlogiston than blood of a lighter colour.

When I had found how readily pieces of blood changed their colour, according to the quality of the air to which they were exposed, I proceeded to examine the state of that air, in order to observe what change had taken place in it; and as dephlogisticated air admits of a more sensible change of quality than common air, I gave it the preference in this experiment; putting a piece of crassamentum, about the bigness of a walnut, into the quantity of about five ounce measures of this air.

This process I continued for the space of twenty-four hours, changing the blood about ten or twelve times; after which I found the air so far depraved, that whereas, at the beginning of the experiment, one measure of it and two of nitrous air occupied the space of no more than half a measure, the same mixtures afterwards occupied

the space of a measure and a half. Now since air is universally depraved by phlogiston, and in this sense, I believe, by nothing else, it is evident, that this black blood must have communicated phlogiston to the air; and consequently its change of colour from black to a florid red must have been occasioned by the separation of phlogiston from it.

The next day, when, of course, the blood was nearer to a state of putrefaction, in which every kind of substance, without exception, will injure respirable air, I put a quantity of red blood, tinged in a few places with black, which I could not easily separate from it, to about the same quantity of the same dephlogisticated air, and suffered it to stand, without changing, for the same space of time; when it was so little injured, that the measures abovementioned occupied the space of only two-thirds of a measure.

That blood has a power of taking phlogiston from air, as well as imparting phlogiston to air, I satisfied myself by exposing blood of a very beautiful florid colour to nitrous air, inflammable air, and phlogisticated air. The two first mentioned kinds of air were considerably diminished by the process, which was continued two days, during which time the blood had been changed five or six times.

The nitrous air, by this means, lost a great proportion of its power of diminishing, that is, phlogisticating, common air. For now two measures of common air and one of this occupied the space of $2\frac{1}{4}$, instead of $1\frac{3}{4}$ measures. The inflammable air, though still inflammable,

was rendered in some degree wholesome by the process; being, after this, considerably diminished by nitrous air, which is a state to which it is brought by agitation in water, and which, continued longer, deprives it of its inflammability likewise. It cannot be doubted, therefore, but that, in both these cases, the red blood, by becoming black, received phlogiston from these two kinds of air.

With respect to the phlogificated air, I only observed that, after a few hours exposure to the red blood, it was sensibly, but not much, diminished by nitrous air, which otherwise it would not have been in the least degree. This blood, however, was of the lightest colour; that is, according to my hypothesis, the most free from phlogiston, of any that I have ever seen; and I have tried the same thing, without success, with blood of a less florid colour, though as florid as the common air could make it. But it should be considered, that the proper function of the blood is not to receive phlogiston from *air*, not meeting with any phlogificated air in the course of its circulation, but to communicate phlogiston to air; and therefore, there is by no means the same reason to expect, that air will be mended by red blood, as that it will be injured by black blood.

It may be objected to this hypothesis, concerning the use of the blood, that it never comes into actual contact with the air in the lungs, but is separated from it, though as ^{Dr. HALE'S} states it, at the distance of no more than a thousandth part of an inch. The red globules also swim

in a large quantity of serum, which is a fluid of a quite different nature.

In order to ascertain the effect of these circumstances, I took a large quantity of black blood, and put it into a bladder moistened with a little serum, and tying it very close, hung it in a free exposure to the air, though in a quiescent state; and the next day I found, upon examination, that all the lower surface of the blood, which had been separated from the common air by the intervention of the bladder (which is an animal membrane, similar to that which constitutes the vesicles of the lungs, and is at least as thick) and likewise a little serum, had acquired a coating of a florid red colour, and as thick, I believe, as it would have acquired, if it had been immediately exposed to the open air; so that this membrane had been no impediment to the action of the air upon the blood. In this case it is evident to observe, that the change of colour could not be owing to *evaporation*, as Mr. CIGNA conjectures. This experiment I repeated, without previously moistening the bladder, and with the very same result.

I observed also, that when I cut out a piece of the crassamentum, and left the remainder in the vessel with the serum, not only that part of the surface which was exposed to the air, but that which was surrounded with serum, and even covered with it to the depth of several inches, acquired the florid colour; so that this deep covering of serum, which must have effectually prevented all *evaporation*, was no more an impediment to the mutual action of the blood and the air, than the bladder had been.

been. The serum of the blood, therefore, appears to be as wonderfully adapted to answer its purpose, of a vehicle for the red globules, as the red globules themselves: for the flightest covering of water, or *saliva*, effectually prevents the blood from acquiring its florid colour; and Mr. CIGNA found that this was the case when it was covered with oil.

That it is really the air, acting through the serum, and not the serum itself, that gives the florid colour to the blood, is clearly ascertained by the following experiment. I took two equal portions of black blood, and put them into equal cups, containing equal quantities of serum, which covered them to the depth of half an inch. One of these cups standing in the open air, and the other being placed under an exhausted receiver, the former presently acquired a florid colour, while the other continued twelve hours as black as at first. Being taken out of the receiver, it stood all night in the open air without becoming red, and continued black ever after, even when the serum was poured off.

I also more completely satisfied myself of the influence of the air upon the blood, through a body of serum, by the reverse of this experiment. For I found that red blood became black through the depth of two inches of serum, when the vessel containing it was exposed to phlogisticated air; so that the red globules of the blood both receive, and part with phlogiston by means of the air, notwithstanding the interposition of a large body of the fluid in which they naturally float.

Except

Except serum, *milk* is the only animal fluid that I have tried, through which the air can act upon blood: for black blood became red when it was plunged in milk, in the same manner as if it had been covered with serum. In urine, indeed, black blood becomes instantly red; but this is not owing to the action of the air, through the urine, but to the saline nature of that fluid.

In some cases, care must be taken to distinguish the floridness with which some detached parts of a quantity of blood are tinged, from that which penetrates the solid parts of it. In *saliva*, and in water impregnated with alkaline salt, fixed or volatile, and also in spirit of wine, the extreme angles and edges of pieces of crassamentum and small detached parts, floating in those liquors, will appear of a very florid red, while the compact mass of blood continues dark. The florid colour of the prominent and detached parts, in these cases, seems to be the mere effect of the minute division of the parts of the crassamentum in the fluid in which those parts float; when at the same time it has no such effect on those parts which remain compact, nor has the air the least power of acting on the blood through the liquor.

I had imagined, that since black blood contains more phlogiston than red blood, that difference would have appeared in the *air* produced from them, either by being simply dissolved in spirit of nitre, or when dried and made into a paste with that acid. But the difference was too small to be sensible to this kind of test. For this purpose, however, I had some blood drawn from the vein

of

of a sheep, and also took some that came first after killing it, as the butchers usually do, by dividing the *carotid* artery; but though I dissolved the black part of the former, and the red part of the latter, in equal quantities of the same spirit of nitre, I found no sensible difference in the air that they yielded. The air that I got from them when dried, and made into a paste with spirit of nitre, was likewise equally indistinguishable. The quantity of air from this process was very great, and was produced irregularly, as I have observed it to have been when produced by a solution in spirit of nitre without drying. *Observations on Air*, vol. II. p. 155. Half of this produce was fixed air, and the rest phlogisticated, except that a candle burned in it with a lambent blue flame. It is evident, however, from this experiment, that even the most florid blood contains a considerable quantity of phlogiston; for, otherwise, this air would have been dephlogisticated.

I would conclude this paper with observing, that I have found a very great difference in the constitution of blood with respect to its property of being affected by the influence of the air; some becoming very soon of a light florid colour, and the *stratum* of this colour soon growing very thick; whereas, in other cases, the colour of the blood, in the most favourable circumstances, has continued much darker, and the lighter colour has never penetrated very far.

As the principal use of the blood seems to be its power of receiving and discharging phlogiston, and the degree in

in which it possesses this power is easily ascertained by the eye, it might not, perhaps, be unworthy of being particularly attended to by physicians. To estimate the goodness of blood, according to this criterion, nothing is requisite but to observe the lightness of the colour, and the depth of the light-coloured *stratum*, after it has been exposed to the air for a given time. In cases in which the blood is unusually black, and but little affected by common air, it should seem, that breathing a purer air might be prescribed with advantage.

In general, the blood that I have been able to procure in the city has not been so good as that which I have got in the country; owing, perhaps, to the cattle having been much driven, and heated before they were killed.

XIV. *Experiments on Water obtained from the melted Ice of Sea-Water, to ascertain whether it be fresh or not; and to determine its specific Gravity with respect to other Water. Also experiments to find the Degree of Cold in which Sea-Water begins to freeze. By Mr. Edward Nairne. Addressed to Sir John Pringle, Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

S I R,

Hampstead,
Feb. 1, 1776.

R. Feb. 1, 1776. **I**T having been suggested, in a conversation at which I was present, that the ice of sea-water is not fresh; and that if the ice found near the poles be really so, it must probably be the ice of fresh water discharged into the sea from large rivers in those parts: I thought the present cold weather afforded an opportunity too favourable to be lost, of ascertaining by experiment, whether the water obtained from the melted ice of sea-water be free from the taste of salt or not; of comparing its gravity with that of the sea-water, &c.; and of finding the degree of cold in which the latter begins to freeze: and I beg leave to lay before you an account of my researches in these matters, and of the methods I followed in making them. If you, SIR, should think them worthy of notice, and would communicate them to the learned body over which you preside, you would confer an honour on, &c.

THE sea-water used in the following experiments was furnished by Mr. OWEN, who keeps the Mineral Water Warehouse, at Temple Bar; who assured me, that it was taken up off the North Foreland.

On the 27th of January, 1766, at ten o'clock in the evening, I filled a jar $3\frac{1}{4}$ inches in diameter and $6\frac{1}{2}$ inches deep, with sea-water, and exposed it to the open air, the thermometer standing at 15° . At noon the next day, on taking it in, I found it frozen very hard, except a very little at the bottom, which remained quite fluid: I now set it by a stove in a heat of 56° to thaw. The ice when taken in from the open air was one quarter of an inch above the edge of the jar. When the jar had continued in the degree of heat abovementioned during eight hours, I took out the ice, which was then $3\frac{1}{2}$ inches long and two inches in diameter; about two-thirds of the water appeared to remain. In order to clear the ice from any brine that might adhere to it, I washed it in a pail of pump water, in which it was suffered to remain about a quarter of an hour, and then set it in a sieve to drain off the water in which it had been washed.

On the 29th of January, 1776, I set the beforementioned ice in a basin in a heat of about 46° , in which it continued nine hours before the whole was dissolved. The bulb of a thermometer rested on the ice during the time of the solution, and continued without variation at 32° . The water thus obtained was, to my palate, perfectly free from any taste of salt.

In

In order to ascertain the comparative gravity of this water, I filled a bottle with it to a certain mark in its neck, which was very narrow, and weighed the bottle so filled very carefully. I weighed the same bottle, filled to the same mark in its neck with sea-water and other waters successively, which were all brought to the same degree of heat by a thermometer. The results were as follow; *videlicet*,

	Grains.
Water obtained from the melted ice of the sea water,	1614
Distilled rain water,	1612
Water taken out of a water tub, being a mixture of rain and snow water,	1615
The sea water,	1653
The <i>residuum</i> of the sea water from which the ice before mentioned had been taken,	1659

To find the degree of cold in which sea water begins to freeze, I made the following experiments.

I exposed to the open air a decanter filled with the sea water, in which a thermometer was suspended, the bulb of which reached to the middle of the widest part of the decanter; a jelly glass filled with the same sea-water, in which also a thermometer was put, resting on the bottom, was placed in the same exposure. The result will be seen in the following table:

January 29, 1776.

Vessel.	Time.	Immerfed Therm.	Therm. in the open Air.	Effects, &c.
Decanter, Jelly glafs,	11 30 A.M.		19	
Decanter, Jelly glafs,	12 0	33 25 to 28.5	19	A number of beautiful feathered cryftals appeared in the jelly glafs; they began to fhoot from the top, which was covered with ice, toward the bottom; when they reached it, the thermometer fole immediately from 25 to 28.5.
Decanter, Jelly glafs,	12 15	31 28.5	19	Ice began to form in the decanter, though hardly perceptible at the edge of the water.
Decanter, Jelly glafs,	12 20	30 28.5		
Decanter, Jelly glafs,	12 30	29 28.5	19	Cryftals of a laminated appearance began to fhoot downwards obliquely from the ice at the furface, which at the edge of the water was barely two-tenths of an inch thick; no appearance of ice in the middle of the furface.
Decanter,	1 0 P.M.	27.5	19	Cryftals began to fhoot round the neck of the decanter clofe to the glafs.
Decanter,	1 15	28.5	19	The infide became covered with finely feathered cryftals, which made it impoffible to obferve the height of the thermometer, without raifing it till the quick-filver in the tube appeared above the ice.
Decanter,	1 4 0	28.5	19	

January 29, at eight o'clock in the evening, I exposed to the open air two similar jars, each $5\frac{1}{2}$ inches deep and $1\frac{7}{16}$ inch in diameter; one of which I shall, for the sake of distinction, call A; the other, B. A was filled with the sea water; B with water taken out of a water tub, which was a mixture of rain and snow water. In A two thermometers were placed; one rested on the bottom; the upper part of the ball of the other was a quarter of an inch only below the surface of the water; one thermometer was also placed in B, resting on the bottom. The following table shews the result.

Vessel.	Time.	Therm. at the Top.	Therm. at the Bottom.	Therm. in the open Air.	
A	h /	60	60	19.5	
B	8 o P.M.		60		
A		40	33		
B	8 15		38		
A		35	29.5		
B	8 20		37.5		The surface of the water in B covered with ice.
A		31	26.5		
B	8 25		34		Surface as before.
A		29	25		No appearance of ice.
B	8 30		32		The ice on the surface increased.
A		28.5	24.5		Ice began to appear on the surface.
B	8 32		32		Quite frozen.
A		28.2	28.5		Crystals over every part of the glass.
B	8 36		32	20	As before.

N. B. During the time in which these observations were made, the thermometer in the open air rose half of a division.

The following table shews the result of some further observations on the effects of cold on the sea-water in the jar A of the last table, which had been thawed in order to be now exposed again to the open air. The thermometers in the jar continued in the same situation as before.

January 30, 1776, A. M.

Time.	Therm. at the Top.	Therm. at the Bottom.	Therm. in the open Air.	Effects, &c.
h /				
10 32	34.5	35.5	16.5	The water fluid.
10 39	29	32		Ice began to be formed about the glass at the edge of the water.
10 42	28.5	30.5		Still continued to have ice only about the edge of the water.
10 48	28	28		The surface of the water rendered stagnant by the ice.
11 1	27	24.5	18.5	The crystals had almost reached the bottom.
11 1½ 2 }	27 +	28.5		During the half minute employed in this observation, the crystals reached the bottom of the jar; the lower thermometer rose almost instantaneously from 24.5 to 28.5, and was immediately rendered obscure by the ice.
11 45	26.5	28.5	19	The jar was taken in from the open air, and the lower thermometer lifted out of the ice to a sufficient height for the observation.

From these observations it seems that the freezing point of sea-water should be fixed in FAHRENHEIT's scale at 28.5.

As the water, when it began to freeze in two experiments, exhibited phenomena different from any I had observed before, it may not be improper to subjoin an account of them.

At

At fourteen minutes after eight in the morning of January 31, I put the jar B of the second table, containing the same water; *viz.* a mixture of rain and snow water, in a window, having the evening before placed a second thermometer in it, the bulb of which was just below the surface of the water. This as well as the thermometer at the bottom stood at 27.5 , and the water was perfectly fluid; the thermometer placed near the jar within the window was at 23.5 . At twenty-seven minutes after eight it began to freeze at the bottom of the jar, the thermometers at the top and bottom standing alike at 27 . The instant the crystals began to encompass the ball of the thermometer below, which they very soon did after it began to freeze, the quicksilver rose in it to 32° , the upper one continuing at 27° . The crystals continued to shoot upward, and in less than half a minute reached the bulb of the thermometer at the surface, which immediately rose to 32° .

At ten minutes before six in the evening of the same day, I put the jar A of the second table into the open air, its contents the same; *viz.* sea water. The thermometers in it were likewise the same, not having been moved; they both stood at 34° ; that in the open air at 19.5 . At six o'clock the thermometer above was at 31° , that below at 28.25 . At this time I discovered some ice on the surface of the water; but as it was by candle-light, I could not discern its first appearance. At ten minutes after six, the thermometer above was at 29° ; that below at 26.5 . At fifteen minutes after six, the upper thermometer at

28.5; that below at 25°. At seventeen minutes after six, both the thermometers stood at 28.5, crystals having risen from the bottom covered the ball of that below, on which it rose instantly from 25° to 28.5. The thermometer in the open air continued as at first; *viz.* at 19.5.

The scale of all the thermometers used in these experiments was FAHRENHEIT'S. I have sent herewith specimens of the water; *viz.* of the sea-water; of the water procured from its melted ice; and of the *residuum* of the sea-water from which the ice was taken.

XV. *Easy Methods of measuring the Diminution of Bulk, taking place upon the Mixture of common Air and nitrous Air; together with Experiments on Platina.* By John Ingenhoufz, M. D. F. R. S. Physician to their Imperial Majesties at Vienna. In a Letter to Sir John Pringle, Bart. P. R. S.

TO SIR JOHN PRINGLE, BART. P. R. S.

S I R,

Vienna,
Nov. 3, 1775.

R. Feb. 15, 1776. **S**OME time ago I amused myself with some experiments relating to nitrous air. Having received from the learned Abbé FONTANA a copy of a pamphlet, which he published this year under the title, *Descrizione e usi di alcuni stromenti per misurare la salubrità del aria, di Felice FONTANA. In Firenze, l'anno MDCCCLXXV: per Gaetano Cambiagi Stampatore granducale,* which most probably will already be known to you; I imitated some of them, and found them very useful for the intended purpose of measuring the quantity of air absorbed or diminished by mixing the nitrous with the common air; by which *criterion* the degree of the salubrity of common air may be ascertained according to the discovery of Dr. PRIESTLEY. Abbé FONTANA first produces nitrous air in a separate vessel, and then forces it into the glass, or other vessel, in which it is to remain, till a communication be opened between this vessel and the

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other

other which contains common air. I found it a difficult matter to force always just the same quantity of nitrous air into the vessel; because I could never be sure that the nitrous air had dislodged all the common air out of it, or had dislodged always the same quantity of common air. If this quantity is not always just the same, some variety must happen in every experiment; and thus an exact valuation of the quantity of air absorbed cannot well be made. To obviate in some measure this difficulty, and to abridge the experiment by mixing suddenly the two airs together, I contrived the instrument of which I send you here a drawing. It is a strong glass vessel, nearly two inches and a half in diameter, and about as much in height: a conical figure would perhaps be better. A brass cover, which embraces the glass about half an inch downwards, is cemented to it, and has a hole in its middle, corresponding with the hole in the glass vessel. This hole of the brass cover has a female screw fitted to receive the male screw of a brass tube, about seven inches long and about an inch in diameter, terminating at one end in a male screw (adapted to the abovementioned copper plate) and at the other, in a neck adapted to enter the mouth of an elastic gum bottle, otherwise called *boradchio* or *caout-chouc*, to be tied to it with a strong ribbon. This brass tube has towards each extremity an air-tight cock, by which the communication between one extremity and the other may be opened or shut. Between these two cocks, about the middle of the tube, is a short lateral tube, communicating

nicating with the canal of the other tube. This lateral tube has also an air-tight cock, which opens or shuts up the communication with the long tube, and has a female screw to receive the male screw of another short tube, which serves to receive a glass tube bent at right angles and of two feet or more in length; the diameter somewhat more than that of a large quill. This glass tube is to be divided into any number of equal parts. I use the instrument in the following manner. The elastic gum-bottle being well tied to the brass tube, all the cocks shut, and the glass tube fixed to its place, I pour a certain quantity of *aqua fortis* (v. g. $\frac{3}{4}$ lb) into the glass vessel, taking care that none of it touches the brass cover: then I put into it a certain quantity of iron filings (v. g. $\frac{3}{4}$ j) wrapt up in a bit of paper to prevent its being immediately corroded. This being done, I screw the glass vessel to the brass tube, so that no air can get out. When the red fumes begin to rise, I open the two cocks of the brass tube, which open the communication between the glass vessel and the elastic gum-bottle. By squeezing the elastic gum-bottle, I force the two airs to mix together. The diminution of the air is soon perceived by the elastic gum-bottle becoming flaccid. When I judge the air is as much diminished as it can be, I put the extremity of the glass tube into a vessel with water, and open the cock of the side tube: the water immediately rises in the glass tube to a height proportioned to the diminution of the two airs. By repeating several times the experiment in the same place, I found the

rise of the water nearly the same, though not so exactly as I could have wished: the variation I ascribed partly to the elastic bottle not being always of the same firmness or elasticity, which it loses more or less by squeezing. I contrived another method more simple, and perhaps more accurate, which is the following: I took a glass tube about two and a half feet long, and not quite a twelfth of an inch in diameter; so that a column of quicksilver might slide through the whole without dispersing itself, filling always the whole cavity. I cemented to each extremity a brass ring, that I might be able to shut the opening with my finger without hurting myself. This tube being divided into 100 equal parts, I used it in two different ways; viz. having poured some *aqua fortis* into a little phial, and put to it some filings, I thrust the extremity of the glass tube, into the neck of the phial. A column of quicksilver of about an inch in length occupied that end of the glass tube which was in the neck of the phial. The whole was kept in such a posture that the tube was nearly in an horizontal line, the end which is put into the phial being rather the highest. Care was taken that the tube should not touch the *aqua fortis*. The phial being filled with red fumes and the extremity of the tube surrounded with them, I open and shut alternately the opposite extremity of the tube, so as to allow the quicksilver to advance slowly towards the middle; as soon as the column of quicksilver is arrived at the middle, I take the tube out of the bottle, and shut each extremity with the fore-finger: thus moving the tube upwards and downwards as briskly as can be done with a certainty of keeping both extremities

all

all the while exactly shut. The two airs being thoroughly mixed, I put one extremity into a vessel filled with quicksilver, and withdrawing the finger from the opening, the quicksilver rises immediately within the tube, and shews by its height the exact quantity of air diminished. The other method is this: I tie to the end of the same tube the neck of a small elastic gum-bottle, the bottom of which is cut away: having put some iron filings into a little phial, filled with *aqua fortis*, I put the end of the tube within the mouth of the phial, clapping my hand fast to the orifice of the phial, the loose part of the elastic bottle, so that the nitrous air, rising from the phial, must take its course through the tube. When the whole tube is filled with red fumes, I take it out, and shut the two extremities with my two fore-fingers. Then I put one end of the tube in a vessel with quicksilver, and withdraw both fingers for an instant, to make the column of quicksilver rise within the tube. I apply immediately both fingers; and holding the tube nearly in a horizontal direction, so that the extremity where the quicksilver is may be rather the highest, I open and shut at the same time both extremities, so that the column of quicksilver gradually advances towards the middle. The quicksilver advancing towards the middle, as much common air follows the quicksilver as it forces out nitrous air from the other extremity. As soon as the column of quicksilver is in the middle, I keep both extremities well shut with my fingers, and moving the tube in various ways, I force the two airs to come into mutual contact, and to mix intimately together. Then I put one
extremity

extremity into a vefſel filled with quickſilver, withdraw the finger from within the quickſilver, and obſerve to what height the quickſilver riſes. It requires ſome practice to perform this experiment with dexterity.

Some time ago I got ſome ounces of fine *platina* from Spain, through the means of his excellency Count DIETRICHSTEIN, with which I made ſome experiments. Moſt writers aſſert, that a conſiderable part of the *platina* is attracted by the magnet, but not the whole of it: but by a nice inquiry I found, that every one of the particles obeyed the magnet more or leſs, except ſome transparent ſtony particles; and that even theſe were all magnets in themſelves; or that each particle had two poles, which I could change at pleaſure by the application of magnetical bars. Though their magnetical virtue is always much leſs than that of particles of iron, yet every one had more or leſs of it; but ſome ſo little as not to be perceived but by applying a ſtrong magnet to them when floating upon water. Beſides the flat, ſmooth, and ſhining bright particles, which are alone the true *platina*, I find two other kind of particles among them; *viz.* ſome very ſmall black particles, moſt of which are of an irregular figure, reſembling the iron ſand found in ſome parts of North America; at Teneriffe; near ſome lakes in Italy; in ſome rivers in Tranſylvania, among the gold duſt, which is taken out of them; and in many other places. Some of theſe 'black particles, though few in comparison with the number of the irregular particles, are of a very regular figure; and when ſeen through a good magnifier, ſomewhat reſemble the figure of a melon.

These black particles of both sorts, I find, are attracted by the loadstone, and have each of them two poles, though those of an irregular figure have them more manifestly ^(a). The other particles are of a gold colour; having, in general, more or less of a paleness approaching to the colour of *platina*. Some of these gold particles have the figure of the rest of the *platina*, differing only from them in colour, and in not being so bright, or as it were polished. Others are irregular masses of indeterminate figure having generally a spongy appearance. The most part of these gold particles were evidently attracted by the magnet, and shewed upon the surface of the water their two distinct poles. These gold particles being put upon a piece of charcoal, and the flame of a candle directed upon them by the blowpipe of the chemical pocket laboratory, described by GUSTAVE VON ENGESTROM, published in the English translation of CROWNSTED'S *Mineralogie*, run easily into round balls, which have all the appearance and quality of real gold, except their being in general magnetical or having two distinct poles. I make no doubt but this magnetical quality is owing to some *platina* mixed with the gold. I could never melt a single particle of true shining *platina* by blowing strongly upon it with the blowpipe; the only change they underwent by this operation was to lose their brightness and the greatest part of their

(a) If magnetism is a criterion of iron, there must be iron in the *platina*; but if the rest of this substance be gold, according to some, why should not this be precipitated together with the gold added to it, by the addition of a solution of green vitriol to the *aqua regia* in which the two metals are dissolved?

magnetical virtue. Having filled a small glass tube with that *platina*, I found each end of it attracted both poles of a compass indiscriminately; but being put to a set of magnetical bars, it became a real magnet, having two distinct poles, which I could change at pleasure. I filled another small tube with *platina*, the hollow of the tube being only of such a size, as to allow the particles of *platina* to go in freely. I stuck a pin in each end, and fixed the pins with sealing wax. I directed five or six electrical explosions from three very large jars through the tube; after which, I found the *platina* had acquired no polarity. By looking with a microscope at the outside of the tube, I found the *platina* was much changed, so as to appear one uninterrupted cylinder of metal, all the interstices between each particle being quite, in appearance at least, obliterated and filled with bright metal. The places which were not bright, were become of a black hue, and appeared to be parts of the *platina* not melted; which I found afterwards to be the case. I attempted to shake the particles out of the tube, but I could not succeed. I could only force out some few at the opening with a pin. I separated a little bit of the tube with a file, to push out the cylinder of *platina*; but could not succeed without employing a great force: therefore I beat some part of the tube to pieces with a hammer, and found each particle had undergone a remarkable alteration. All of them appeared in several places to have been melted, and some little ones seemed to have been intirely in a fluid state; they all adhered in lumps together so strongly, that many of them

them could absolutely not be rubbed afunder between the fingers. The inside of the tube exhibited marks of having received impreffions of the melted metal. By comparing the separated particles of this *platina* with particles not expofed to an electrical explofion, they were fcarce to be known for the fame fubftance. I had put fome iron filings in a tube of the fame fize, and directed the fame explofion through it, in order to compare the effect of electricity upon it with what happened to the *platina*. I found, by looking at the outside, fomewhat of the fame appearance of being melted. By cutting this tube in fmall bits, I could eafily push out the filings with a pin, which I could not do in the other cafe but with great force. The filings ftuck together, as the particles of *platina* had done; but with lefs force. By this experiment it fhould feem as if *platina* (which hitherto could never be melted by common fire by itfelf, but only in the *focus* of a very ftrong burning glafs, fuch as was a little while ago made at Paris) were equally fufible, if not more fo than iron, by electrical fire. I was fomewhat furprized to find, that the particles of *platina* taken out of the aforefaid tube, had got a remarkably ftronger magnetical force, being attracted by a loadftone at a greater diftance, and turning their poles more briskly upon the water than before, though the whole cylinder of thefe particles, ftill inclofed in the tube, gave no figns of having acquired polarity. Thus it appears, that common fire diminifhes the magnetical virtue of *platina*, and that electrical fire increafes it; which I thought the more probable,

bable, because those very particles, which had acquired by electricity their increased magnetical force, did lose it again after being heated upon a piece of charcoal, which did not happen in the particles of iron. *Platina* mixed with lead was put upon an ordinary cupel in a docimastic furnace strongly heated. When the metal came to a solid state, it was a flat rough lump, much heavier than the crude *platina*. I put fresh lead to it, and cupelled it again as before. I repeated it ten times, when I obtained a large lump, somewhat less flat, pretty smooth, but not bright; of about the same weight as after the first cupellation^(b). This lump did not give the least sign of magnetism, and even would not receive any by being applied to strong magnetical bars (I forgot to try this after the first cupellation) and the substance was very brittle, nearly of the same colour as *platina*, and took a fine polish. If it could tend to any useful purpose, I would repeat these experiments oftener, to be quite sure whether the event would be constantly the same.

Though a piece of soft iron attracts the two poles of a compass indiscriminately, and is incapable of acquiring polarity itself, yet I have never been able to separate a single particle of the softest iron, even when I separated it carefully with a flint, or other body containing no steel or iron, without its giving evident signs of two distinct poles when floating upon water, nay even upon paper. I could also never find iron filings of ever so soft

(b) I lost the paper that contained the exact weight before and after the cupellation.

a substance, but each particle separately had evidently two poles. Such iron filings mixed with bees wax, as much as is sufficient to keep them together, got a strong polarity by being touched with magnetical bars, and had all the qualities of a magnet: the mass is easily cut with a warm knife, and is very convenient for magnetical experiments, such as Dr. KNIGHT made with similar loadstones made of pounded magnets. I found also, that each particle of those granulated iron ores of Sweden, which are placed among the *minerae ferri retractoriae*, separated iron from stone, and had two distant poles; and that a piece of the ore itself became a tolerable good magnet by being touched with the bars.

I am, &c.

XVI. *An Account of Three Journeys from the Cape Town into the Southern Parts of Africa; undertaken for the Discovery of new Plants, towards the Improvement of the Royal Botanical Gardens at Kew. By Mr. Francis Maffon, one of his Majesty's Gardeners. Addressed to Sir John Pringle, Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

S I R,

Kew, Nov. 1775.

R. Feb. 1, 1776. **I**N compliance with your request, I now send you the account of my first journey from the Cape, which I have transcribed from my journal; and if you shall find it to contain any thing worthy the notice of the Royal Society, I beg you would do me the honour to present it to that illustrious Body; and believe, that with the greatest pleasure I shall communicate to you and to them the remaining part of my observations.

I am, &c.

AN ACCOUNT OF THE FIRST JOURNEY.

ON the 10th of December, 1772, I set out from the Cape Town, towards the evening, attended by a Dutchman, and a Hottentot who drove my waggon, which was drawn by eight oxen; this being the manner of travelling there. They prefer oxen to horses, because they are much cheaper, and more easily maintained. At sun-set we crossed the Salt River, about two miles distance from the Cape Town, where is placed a high flag-staff with a large old piece of cannon, intended to give signals to prevent a surprize from an enemy: these signals are answered by others, placed upon eminences at proper distances, and alarm the adjacent country in a short time. In the night we travelled over a large sandy plain; and towards the morning stopped at a small cottage called Elfis Kraal. The next day we partook of the diversion of hunting a small species of antelope, which the Dutch call Steenbock. We crossed great part of this sandy plain, which is very extensive, reaching from the Tyger Berg to Bay Falso, upwards of twenty miles; from the Table Mountain to Hottentot Holland Mountains, about thirty miles. The soil of this plain is unfit for cultivation; being a pure white sand, blown by the S.E. wind from the shore of Falso Bay, and often forming large hillocks; it is, nevertheless, overgrown with an infinite variety of plants peculiar to this country.

11th, We passed the Tyger Berg, leaving it on our left hand; and along its skirts saw many fine plantations, abounding with corn fields and vineyards.

12th, We passed the Paerden Berg (that is, Horsesh Mountain) so called from the number of Zebras formerly found there, which are called by the Dutch inhabitants wild horses. Towards the evening, crossing the Berg Rivier (that is, Mountain River) we entered into the district called Draakensteen, a valley about ten miles in length, and about five in breadth; containing many large plantations of vineyards, and orchards of most kinds of European fruit, which have been transported hither by the Dutch; *viz.* apricots, peaches, plumbs, apples, pears, figs, mulberries, almonds, chestnuts, and walnuts; but no Indian fruits, except the guyava and jambo, neither of which ripen well. These plantations are generally situated near the foot of the mountains, and watered by small streams, which descend with great rapidity, and are conveyed all over their gardens and vineyards.

16th, We travelled to a small village called Perel, so named from its situation on the N.E. side of a hill called Perel Berg. In it is a church and about a dozen of houses dispersed along the foot of the hill, with pretty gardens and vineyards, which produce excellent wine.

17th, I went up to the top of the Perel Berg, where I spent a whole day in search of plants, and hunting a sort of antelope called Ree Bock; but had no success. I saw nothing here so worthy of observation as two large solid rocks, of a roundish figure; each of which, I may positively

tively say, is more than a mile about at the base, and upwards of two hundred feet high above the ground. Their surfaces are nearly smooth, without chink or fissures, and they are found to be a species of *saxum* or granite, different from that which compose the neighbouring mountains.

18th, From hence we continued our journey to a valley, adjoining the S. E. part of Draakensteen, called Fransche Hoek^(a); it having been settled by a party of French refugees, who left France about the beginning of this century. Though but a poor settlement, being a cold, moorish soil, it produces corn enough for its inhabitants, four wine and some fruit. Drakensteen and Fransche Hoek are bounded on the N.E. and S.E. by a chain of high mountains, which have their beginning at Cape Falso, run in a winding course to the N.W. of St. Helena Bay, and send out several branches into the interior parts of the country. These two valleys are watered by the Berg Rivier, which rises in the Stellenbosch mountains. It is a considerable river, but no where navigable. The banks are decorated with a great variety of uncommon trees.

January 4, 1773, We reached Stellenbosch, a small village about thirty miles N.E. from the Cape Town, consisting of about thirty houses, forming one regular street, with a row of large oak-trees on each side along the front of the houses, which render it very pleasant in

(a) This, I suppose, to be the place which some of the French voyagers in their observations on the Cape of Good Hope, call Petite Rochelle.

the hot season. These oaks, which are of the same sort with ours in England, were brought out of Europe by ADRIAN VANDERSTELL, formerly governor of the Cape, who built this village, and gave it his name. The country round it is populous, and contains many rich farms, which produce plenty of corn and wine. It is watered by a small river called Eerste Rivier, which discharges itself into the East part of Falso Bay. The farmers we found busy in treading out their corn; which is performed by horses in the following manner. They make a circular floor about thirty, forty, or fifty feet diameter, with a composition of clay and cow-dung, which binds very hard; round it they erect a mud wall, about breast high; this floor they cover with sheaves, beginning in the middle, and laying them in concentric circles till they reach the outside. They then turn in about twenty or thirty horses, which a Hottentot, furnished with a long whip, drives round and round till the corn be trodden out, and the straw become as fine as chaff; which they afterwards clean, and carry into their granaries. This method they can practice with great security, as it seldom rains here from the middle of October to the middle of March.

5th, From thence we travelled along the foot of the Stellenbosch mountains to Hottentot Holland, a pleasant and fertile country; surrounded on three sides by the mountains, and the other opening to the East part of Falso Bay. There are eight or ten plantations, with elegant

gant houses, gardens, vineyards, and corn fields: this country lies about thirty-five miles East from the Cape Town.

6th, We ascended the mountains by an exceedingly steep rugged path, which the peasants call Hottentot Holland Kloof^(b), and after much labour and fatigue gained their summit, when we entered a spacious plain, interspersed with an infinite number of large fragments of rocks, visibly decayed by the force of the S.E. wind, which blows here during the summer with very great force. Some of these rocks appeared like the ruins of church-steeple, and were worn so thin with wind and rain, that the softer parts of them were perforated in many places. They are formed of the *cos quadrum* of LINNÆUS. The soil about them is a black earth intermixed with a pure white sand, probably proceeding from the decay of the rocks. These mountains abound with a great number of curious plants, and are, I believe, the richest mountains in Africa for a botanist. We then passed the Palmet Rivier, so called by the peasants from a plant^(c) which almost covers the water; the leaves of which greatly resemble that of the ananas or pineapple, but their flowers are like those of a reed. At night we crossed a small river, called Boter Rivier; and took up our lodging at a mean cottage, where the Dutchmen and Hottentots live almost promiscuously together, their beds consisting only of sheep's skins. The next morning an

(b) Kloof, is a narrow passage over the lower part of a chain of mountains, or sometimes a narrow passage between mountains.

(c) *Schoenus ferratus*.

old Hottentot brought out a fat wether, and slaughtered it; part of which we ate for our breakfast.

8th, We came to a hot bath, situated on the S.E. side of a large mountain called Zwart Berg^(d). The India Company have erected here a tolerable house for the reception of sick people. The water is scalding hot where it springs out of the earth; but after being conveyed about ten or twelve paces to the bath, it becomes more temperate. The people here seem to use it for all diseases without exception, and often perhaps receive more hurt than benefit by it.

10th, We crossed Rivier Zonder Eynde; that is, Endless River, which discharges itself into the Breed Rivier^(e). At night we came to Sweet Milk Valley, where there is a good house belonging to the overseer of the Company's woods; who received us with great civility, and kept us with him five days. The fourth day, we went into the woods, which are about half-way up a high chain of mountains that extends along the N. and N.E. side of the valley. I was accompanied by a farmer's son, who took with him eight large rough dogs, which in our way started two wolves; one of them we wounded with small shot, so that the dogs overtook him. A fierce battle ensued, which lasted an hour before he was killed. We afterwards climbed over many dreadful precipices till we arrived at the woods; which are dark and gloomy, interspersed with climbing shrubs of various kinds. The trees are very high; some from eighty to a hundred feet;

(d) Black Mountain.

(e) Broad River.

often growing out of perpendicular rocks where no earth is to be seen. Among these the water sometimes falls in cascades over rocks two hundred feet perpendicular, with awful noise. I endured this day much fatigue in these sequestered and unfrequented woods, with a mixture of horror and admiration. The greatest part of the trees that compose them are unknown to botanists. Some I found in flower; others, which were not so, I was obliged to leave for the researches of those who may come after me in a more fortunate season.

16th, I visited a Hottentot Kraal: the men were all, at this time, attending their herds; but the women and children were employed in building their huts; which are very low, of a circular figure, and made of slender poles, the ends of which are stuck into the ground, so as to form a number of arches crossing one another; these they afterwards cover with mats made of reeds. They have a round hole in the middle of the floor, in which they make the fire, and sit all round it upon the ground; but have no chimney or hole to let out the smoke.

18th, We crossed the Breed Rivier, which is considerable, and only passable in winter by a ferry; but at this season we forded it at the place where the Rivier Zonder Eynde joins it. At night we arrived at Schwellendam, a place about 150 miles N.E. from the Cape Town, where we remained two days; but finding the season too far spent for making any considerable collections, I returned back to the Cape by the same road I came. It was on this

journey that I collected the seed of the many beautiful species of *ericæ* which, I find, have succeeded so well in the Royal Garden at Kew.

S E C O N D J O U R N E Y.

R. Feb. 22,
1776. **M**Y second journey was performed in company with Dr. THUNBERG, a native of Sweden; who was sent out by the Dutch to collect plants at the Cape, and is on that errand now in the Dutch East Indies.

Sept. 11, 1773, We left the Cape Town, directing our course along the N.W. Coast. We passed the Blue Mountains; but the weather proving rainy, and attended with a fog, we lost our intended road, and were obliged to lodge that night in the fields.

12th, We came to Groene Kloof, a place belonging to the East-India-Company, where we remained several days, the weather being rainy and unsettled; during which time we made several excursions along the sides of the hills, and also over a large sandy desert towards the sea shore; where we found a great variety of beautiful plants, and several animals peculiar to this climate; *viz.* antelopes, ostriches, and plovers of several sorts.

13th, From Groene Kloof we passed a small hill, called Konter Berg; and from thence entered a large barren country, named Zwart Land^(a). The earth is a grey sand,

(a) Black Land.

level

level for many miles, and covered over with low shrubs of various sorts. At night we came to a farmer's house, where we remained two days, ranging the adjacent fields, in which we found many curious plants, and shot several animals, as steenbocken, hares, partridges.

22d, Still, in the same direction, we travelled over a deep, sandy country with great fatigue; when, towards the evening, we arrived at Saldana Bay. Here we lodged with a farmer on the East side of the bay; but being desirous of crossing to a house inhabited by some of the company's servants, who during the summer season shoot seals for oil, on the 23d we made a signal for them to send their boat; which they immediately answered, and brought us over the bay, which is about two miles in breadth, where we were hospitably entertained by the master. I observed, that the direction of this bay is laid down wrong in all the maps that I have seen, except that of the Abbé DE LA CAILLE; they have given it a right East direction, whereas it has nearly a South direction, almost parallel to the sea coast, and, I suppose, almost twenty miles in length. The entrance of the bay is difficult, having several small islands in it, and the adjacent country being little better than a sandy desert, and the water brackish; it is, I think, improper for shipping. It lies about fifty miles N.N.W. from the Cape Town. We found here great variety of curious plants; and in particular, a large bulbous root, growing on dry precipices, which the Dutch call *vergift-boll*, poison bulb; the juice of which, they say, the Hottentots use as an ingredient to poison their arrows.

We

We found it to be a species of *amaryllis*, and, by the leaves growing in a fan shape, we called it *amaryllis disticha*.

27th, From Saldana Bay we journeyed to Witte Klip (White Cliff) being a white granite stone of an enormous size; from the top of which we had a charming view of the sea coast from St. Helena Bay to the Cape of Good Hope. The whole country affords a fine field for botany, being enamelled with the greatest number of flowers I ever saw, of exquisite beauty and fragrance. Here we saw numbers of wild dogs, and some of them so near that I could discern them to be about the size of a large fox-hound. They go in large packs, and do great damage to the cattle. They also destroy the antelopes wherever they go, by hunting them down in the same manner as our hounds do a stag.

30th, To St. Helena Bay, where the Berg Rivier discharges itself, which is here very deep, and bordered on each side by extensive marshes that are impassable, and overgrown with very high reeds. Those reeds are plentifully stocked with birds of various sorts, which build their nests upon such of them as hang over the water. There is one bird, in particular, which has a wonderful effect among the green reeds; its body being a bright crimson, with black and grey wings; and by the brightness of their colours, when sitting among the reeds, they look like so many scarlet lilies: this is the *loxia orix* of LINNÆUS. There are still some of the sea horse, or *hippopotamus amphibius*, in this river; but it is now prohibited to shoot
any

any of them, as they are nearly destroyed for 800 miles from the Cape. The farmers shoot them for their flesh, which they esteem as good as pork; and of their hide, which is extremely thick, they make whips. There happened at this time a great flood, that prevented our crossing the river at this place, and obliged us to travel four days up the river to a ferry, which greatly retarded our journey, and occasioned many difficulties by the deepness of the sand and brackishness of the water; nor is there any wine or fruit in this part of the country, owing to the saltness of the soil.

Oct. 6th, We came to the pont or ferry, where we collected a great number of beautiful plants, particularly *ixiæ*, *irides*, and *gladioli*.

7th, We crossed the Berg Rivier, and entered a fine plain country, called 24 Rivieren District; so called from the number of small rivulets which run through that district, and discharge into the Berg Rivier. Here we had some four wines, and oranges and lemons in great plenty.

9th, We passed a branch of that chain of mountains which I mentioned in my first journey. They continue for many miles further to the N.W. gradually diminishing in height to the Western shore. This passage over the mountains is called Kartouw, and is remarked for being one of the most difficult in this part of Africa; which we found true, being obliged to lead our horses for three hours amidst incessant rain, which made the road so slippery that, by often tumbling among the loose stones, they had their legs almost stripped of the skin;

skin; and the precipices were so steep, that we were often afraid to turn our eyes to either side. Towards sun-set, with great labour and anxiety, we got safe to the other side, where we found a miserable cottage belonging to a Dutchman. Being however cold and wet, we were glad to take refuge under his roof. The hut had only one room; but our host gave us a corner to sleep in, which was detached by a hanging of reed mats, where he and his wife also slept; and in the other end lay a number of Hottentots promiscuously together.

10th, We crossed the Olyfant's Rivier, nearly 130 miles North of the Cape Town, where we entered into a pleasant valley, bounded on each side by very high mountains; those on the East had their summits covered with snow, it being then their spring. This country produces good corn and European fruit in great plenty, especially oranges and lemons in the greatest profusion; and the trees grow to a great size. They have also wine, but it is sour and unwholesome; which, I think, may be owing to their planting their vines in wet, marshy places. The fruit yields watery juices, which seldom ripen, but produce good brandy. There is a hot bath here, which we visited, issuing from the side of a mountain. The water was nearly boiling hot at the place it issued out of the rock; and the people who used it affirmed that it was not enough to boil a piece of meat. I observed many orange trees, which had been either raised from a single seed, or planted when very young, in a seam of the rock, where the water boiled out, which, to

my surprize flourished amazingly, and all the sides of the basin where the people bathed were matted round with the fibres.

11th, 12th, 13th, 14th, We travelled along the banks of this river, making short stages. The meadows yielded excellent pasture for our cattle, the grass reaching up to their bellies, but of a coarse texture, being chiefly *Juncus*, *Scirpus*, and *Cyperus*.

15th, We attempted to cross the high ridge of mountains on the North side; but found it impracticable, having overturned our waggons on the side of a precipice, and greatly damaged them, which obliged us to return to a peasant's house to get them repaired. This done, we held a consultation what course to take; and after some warm debates, concluded to send our waggons round to a place called Rood Land, there to wait for us, while the Doctor and I directed our course through a country called Koud Bocke Veld, or Cold Country of Antelopes; so named from a species of antelopes which inhabits here, called Spring bock. This animal when hunted, instead of running, avails itself of surprising springs or leaps, which I shall have occasion more particularly to mention hereafter.

17th, We directed our course Eastward through Elans Kloof, a narrow winding passage through a high chain of mountains, which lies to the N.E. of Olyfant's Rivier. This road is rugged beyond description, consisting of broken and shattered rocks and rugged precipices, encompassed on each side with horrid impassable moun-

tains; the sides of which are covered with fragments of rocks that have tumbled down from the summits at different times. We saw few plants here, only some trees of the *protea grandiflora* thinly dispersed along the skirts of these mountains. We crossed, in this passage, several small rivers of the purest water I ever beheld, which afforded us no small relief during the heat of the day. Towards the evening we entered the Koud Bocke Veld; and afterwards came to a peasant's house, where we remained that night.

18th, 19th, 20th, We travelled through the Koud Bocke Veld, where we found but few plants: the face of the country being exceedingly barren, and not so much as a shrub to be seen. The season here appeared to be two months later than in the neighbourhood of the Cape Town, although the distance be not above a hundred miles, in a direct line in a Northern direction. This country is but small, containing about nine or ten Dutch places, the inhabitants of which subsist intirely by their cattle. Their winters are often so severe, that the ground is covered with snow for ten days together; and their early calves and lambs are often killed by the inclemency of the weather. Neither orange trees nor vines will live here, owing to the bleakness of its situation; and the boors informed us, the summers are often so unkindly, that their wheat is blighted while in ear, so that they purchase corn with their cattle from the low country farmers. The country is encompassed on all sides with very high mountains, almost perpendicular, consisting of bare rocks, with-

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out the least appearance of vegetation; and upon the whole, has a most melancholy effect on the mind. We saw some herds of the spring-bocks, a species of antelope, as observed before, which were so shy, that we could not come within musket-shot of them.

21st, We descended by a very steep path into another small country, called Warm Bocke Veld, encompassed also on all sides with horrid mountains, but not nearly so barren. Here we had some four wine and fruit; we were also delighted to see the luxuriance of the meadows, the grass reaching to our horses bellies, enriched with great variety of *ixiæ*, *gladioli* and *irides*, most of which were in flower at the Cape in the month of August.

22d, We had a high chain of mountains to pass before we arrived at Rood Land, where we expected to meet our waggons. Upon inquiring about the road thither of the women, with whom we had lodged the preceding night, the men being all from home, so that we could not procure a guide; they informed us, there was only one pass, called Mostart's Hoek, which was very dangerous; and that, without a guide, we should run the risk of losing our lives, having a rapid river several times to cross, the fords of which, by the late rains, had been rendered more dangerous than usual. We were a little intimidated by this information; but fortifying ourselves with resolution we proceeded, and in an hour arrived at the first precipice, where we looked down with horror on the river, which formed several cataracts inconceivably wild and romantic.

romantic. This pass, which took us near three hours march, is at the broadest about a quarter of a mile, but in general not above an eighth part of one. The mountains on each side rising almost perpendicular to a stupendous height, had their summits then covered with snow, part of which remains till March. This river, which is the beginning of the Broad River, we had four times to cross. The ford was exceedingly rough, the bed of the river being filled with huge stones, which tumble down from the sides of the mountain; but we thought our labour and difficulties largely repaid by the number of rare plants we found here. The bank of the river is covered with great variety of evergreen trees; viz. *brabejum stellatifolium*, *kiggelaria Africana*, *myrtus angustifolia*, and the precipices are ornamented with *erice* and many other mountain plants never described before. At night we arrived at Rood Land, where we found our servants and waggons, and being a little fatigued we devoted the next day to rest and the examination of our plants. It is to be observed, that during the preceding five days we had rather shortened our distance from the Cape, by reason of the impossibility of taking the waggons over the mountains with us; so that we were now one day's journey nearer the Cape than we had been on Bocke Veld.

16th, We travelled up a high mountain, called Winterhoek, on the N.W. of Rood Land, one of the highest mountains in this part of Africa, whose top is covered with snow throughout part of the year. Here we expected to find plants that might endure the severity of our

our climate; but when we arrived at its top, we found nothing but a few grasses, *restiones*, *elegiæ*; the whole mountain consisting of rock, lying in horizontal *strata*, without any sort of earth, except a little decayed rock in which the grasses grew. From the foot of this mountain to its summit is a good day's journey, it being very rugged and difficult to mount. We found many curious plants growing along the borders of the streams, which run in great plenty down the mountain's side. Rood Land is a fine level country, surrounded on all sides by lofty mountains, except on the East, where the valley continues for several days journey inclosed by mountains on each side. Those on the Northern side continue for several hundred miles in an oblique direction, and terminate on the Eastern coast. This country produces corn and wine in abundance, and most of our European fruits, which have been planted there by the new inhabitants, who are descendants of the French refugees; a civil, hospitable, and industrious people.

28th, 29th, We continued our journey along the banks of the Broad River, where we collected many remarkably fine flowers, particularly one of the lilaceous kind, with a long spike of pendulous flowers, of a greenish-azure colour, which among the long grass had an admirable effect (this is *ixia viridis*).

30th, We crossed the Hexen Rivier (Witches River), which has a passage through the mountains, and joins

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the Broad River; this place is also remarkable for a hot bath.

31st, We passed on to Ko Aree Rivier, where we found many new plants; in particular, *gerania* and *flapelia*.

Nov. 2d, To Koekman's Rivier, the banks of which are covered with thick woods, and furnished with a variety of birds, which afforded us good sport. The trees were mostly of the *mimosa nilotica* of LINNÆUS; the species of the birds I have not yet determined, not being provided with books upon Ornithology to settle one half of those which I collected on this journey.

5th, We arrived at Swellendam, described in my first journey; and the same day dined with the Land Droft, who is a justice of peace, and collects different taxes from the peasants. After dinner we pursued our route to Buf-fel Tagt's Rivier, where is a place belonging to the East India Company. There they keep a few wood-cutters, and from thence supply the wheelers, at the Cape, conveying their wood in waggons drawn by oxen: this place, I think, is about 150 miles from the Cape. Here we rested five days for the benefit of our oxen, which had become very lean, and the Doctor got a fresh set out of the East India Company's herd.

10th, To Davenhoek's Rivier, where we remained all night, and the next morning proceeded on our journey. The Doctor imprudently took the ford, without the assistance of the Company; when on a sudden, he and his horse plunged overboard, and ears into a pit, that had been made by the *hippopotamus amphibius*, which formerly inhabited

habited those rivers. The pit was very deep, and steep on all sides, which made my companion's fate uncertain for a few minutes; but, after several strong exertions, the horse gained the opposite side with his rider.

12th, To Caffer Kuyl's Rivier. Upon our left hand, a few miles distant, we had the chain of mountains before mentioned, which here take a N.E. direction. Their summits terminate in a number of lofty, rugged picces, which have an admirable effect. Between this chain of mountains and the sea on the S.E. lies an extensive country, to appearance low; but when one travels across it, it presents a continued series of hills and dales. The hills are quite smooth and easy of ascent, and covered with long coarse grass, which cattle seldom eat. On the declivities of these low hills grows the *aloe Socotorina* in large clumps, which when old have stems about five or six feet high, with only a few thick leaves on their tops, that at a distance appear like bands of Hottentots. The peasants make great quantities of the gum aloes from the sap of the leaves, which they sell at the Cape from two to six pence *per* pound. There is a fine species of antelope, which inhabits only here, called by the peasants Bonte Bock; something larger than a fallow deer, very shy, but not very swift.

15th, To Goud's Rivier; which at that time was about 100 yards broad, and the water came up to the seat of our saddles. On each side of this river lies an extraordinary track of land, which in the Hottentot language is called Carro. It is a dry, burning soil, of a reddish colour, intermixed

intermixed with rotten rock, and intirely divested of grafs; but enriched with an infinite number of evergreen shrubs, both frutescent and succulent: among the latter we found many new species of *crassula*, *cotyledon*, *euphorbia*, *portulaca*, *mesembryanthemum*. We resolved to visit the sea shore, and particularly Mossel-Baay; when, late in the evening, we came to the house of an European, who received us very hospitably. He was a native of Swedish Pomerania, about seventy years old; had been shipwrecked on the coast of England fifty years ago, and spoke much of the hospitality of the English. He was a man of learning, and expressed many sensible reflexions on the tyranny of his native country, which had forced him to seek for an asylum in the deserts of Africa. His house was very mean, built of mud, and miserably furnished; not having a bed to lie on, though he had several hundred oxen and some thousands of sheep. He had a number of Hottentot vassals, whose huts were situated round his folds, where they kept several large fires all night long, to frighten away the wolves and tigers.

16th, We came to Mossel-Baay, which is very large, open, and exposed to the S.E. and E. The shore is covered with shrubs of various kinds; the greatest part of which were unknown to us, and many we did not find in flower. To the N.E. of Mossel-Baay lies a woody country, called Houtniquas Land; whose woods, intercepted by rivers and precipices, are so large, that their extent is not perfectly known. These woods are a great treasure to the Dutch, and will be very serviceable to the

inhabitants of the Cape, when their other woods are exhausted. In them are numbers of wild buffaloes that are very fierce, and some elephants; which renders travelling dangerous. We now directed our course Northward to the foot of the great chain of mountains, which we had again to cross; it is there very broad, being a hard day's march from one side to the other. This pass is called by the peasants Hartiquas Kloof.

19th, We were several hours in ascending, and after descending on the other side, we entered a valley, surrounded by lofty mountains: here we rested that night by a stream of water, where we collected many curious plants.

20th, We continued our journey through a dismal valley, where we saw neither man nor beast; but our labour was generously rewarded by the productions of the vegetable kingdom, having found several new species of plants, which for neatness and elegance exceeded any thing I had ever seen. At night we got clear of the mountains, but entered a rugged country, which the new inhabitants name Canaan's Land; though it might rather be called the Land of Sorrow; for no land could exhibit a more wasteful prospect; the plains consisting of nothing but rotten rock, intermixed with a little red loam in the interstices, which supported a variety of scrubby bushes, in their nature evergreen, but, by the scorching heat of the Sun, stripped almost of all their leaves. Yet notwithstanding the disagreeable aspect

of this tract, we enriched our collection by a variety of succulent plants, which we had never seen before, and which appeared to us like a new creation.

21st, To Great Thorn River, where we encamped under a large *mimosa* tree. During the night, we had several loud claps of thunder with rain.

22d, We entered Lange Kloof, which is a narrow valley, not exceeding two miles at the broadest, and in length about 100; bounded on the S.W. by the chain of mountains beforementioned, and on the North and East by a lower ridge, which runs nearly parallel. It contains about seven or eight places, which are from twelve to twenty miles distant from each other; the houses are very mean, without walls, consisting only of poles stuck in the ground, meeting at the top, and thatched over with reeds. The people, however, are wealthy, possessing large herds and flocks. The Hottentots are in general servants to the Dutch farmers; who give them for wages beads, and tobacco mixed with hemp; the latter, which intoxicates them, they are extremely fond of. A few free Hottentots still remain here, who live in their ancient manner; but who are miserable wretches, having hardly any stock of cattle.

29th, To Kromme Rivier (that is, Crooked River) a long, marshy vale, which lies much lower than the former, and is bounded by a continuation of the above-mentioned mountains.

30th, To Effé Bosch, where we encamped that night in the open fields, clear of the woods, for fear of the lions.

Dec. 1st, We entered a fine level country, bordering on the Eastern Ocean, leaving behind us the chain of mountains before mentioned, which runs obliquely across the country from the Atlantic to the Indian Ocean. At night we came to Zee-Koe Rivier, or Sea-Cow River, so called, erroneously, from the *bippopotamus amphibius*, which formerly inhabited it, but is now almost extirpated. We rested here eight days; in which time we ranged the adjacent woods and fields, where we greatly increased our collection. The river was frequented by a variety of water-fowl which afforded us good sport: there were numbers of the *phenocopterus ruber*, *pelicanus onocrotalus*, with many others, which we could not class, being unprovided, as I said, with books of Ornithology. We lodged at the house of JACOB KOCK, an old German, who used us with great civility. He had built a handsome house, made gardens and vineyards, possessed numerous herds of cattle, and had upwards of a hundred Hottentots in his service, whom he employed in taking care of them. The face of the country changes greatly, being open, plain, and covered with verdure, extending many miles along the sea-coast, containing several tribes of Hottentots. The rivers formerly abounded with the *bippopotamus amphibius*; but since the Dutch inhabited these parts, they have almost destroyed them. They shoot

them for their flesh, which they esteem equal to pork, their fat being much of the same quality. The manner in which the Hottentots catch these animals is as follows: the banks of the rivers, as I have already observed, are covered with almost impenetrable woods; these animals in the day time lodge themselves in the deepest places of the river, and when night comes, make excursions into the adjacent fields to graze, taking their course through paths, which they have made in the woods. In these paths the Hottentots dig large pits, which they cover over with boughs of trees and grass; then hunting them out of the fields, the animals make full-speed towards the river, and fall into these pits; from whence they are unable to get out, on account of their great weight, and then the men come up with their lances and kill them. We found here a new palm, of the pith of which the Dutchman told us the Hottentots make bread; but we could get no satisfactory account of their method of making it. We observed two species; one about a foot and a half diameter in the stem, and about twelve feet high, with entire leaves; they appeared to be very old, and seldom bore fruit. The other sort had no stem, with the leaves a little serrated, and lying flat on the ground, which produced a large conical fructification about eighteen inches long, and a foot or more in circumference; squamose, and under each of the *squamæ*, is an oval nut, about the size of a chestnut, of a beautiful red colour, but insipid taste. The male plant is similar to

to the female, only not producing fruit, but bearing a *strobilus*, and containing the *pollen*, or male-duft, in small cells underneath its *squamæ*. In the woods here we found the *euphorbia antiquorum* forty feet high. The inhabitants observe, that the honey found near these trees is unwholesome. Being still determined to continue our journey about 150 miles further, directing our course towards the middle of the country, and to return to the Cape another way; I furnished myself with a set of fresh oxen and a fortnight's provision; and Mr. ROCK gave us one of his sons for a guide and to serve us as interpreter, he being a perfect master of the Hottentot language.

9th, We took leave of our hospitable friend, and departing towards the evening, we stopped that night at the house of JACOB VAN RENNEN, a wealthy grazier: this was the last Dutch place in this part of the country: From hence we travelled through a rugged hilly country, covered with thick coppices of evergreen trees; but the way was so rough that our waggons were almost shaken to pieces. Towards noon we crossed Camtour's River, where we rested during the heat of the day, and amused ourselves in the woods along its banks, which were extremely pleasant: the river is broad and deep in many places. The woods are frequented by elephants, buffaloes, and lions; and the deepest parts of the river by the *hippopotami*. We found many new plants here, notwithstanding our stay was so short. In the afternoon we advanced through

through a woody country, where we observed numbers of butterflies, which appeared like those of India; but from the thickness of the woods we could not procure a single specimen. At night we came to Lory's River, so called from a species of parrot, which is found here. We were visited by several Hottentots, who came out of the woods armed with lances, but behaved very obligingly, and slept by our fire all night; and we at the same time entertained them with tobacco, of which they were exceedingly fond.

11th, We travelled over a pleasant country, diversified with smooth green hills, interspersed with evergreens, and stocked with numerous flocks of the *capra dorcas* of LINNÆUS, *equus zebra*, and *camelus ferus*; which, together with the fine disposition of the woods and groves, could not but charm us, who, for upwards of three months, had been climbing rugged mountains, and crossing sultry deserts. In the evening we came to Van Staad's Rivier, where we remained all night, and were visited by several Hottentots, who brought us milk in baskets made of fine reeds, which they weave so close that they hold any liquid.

12th, We crossed Van Staad's Rivier, where there is a large Kraal, or Hottentot village, containing upwards of 200 inhabitants, who are possessed of great herds of bullocks, but of no sheep. These Hottentots were remarkably well-shaped, and stouter made than any other Hottentots I have yet seen. They are also very bold in

encountering wild beasts, particularly the lion, which often attacks their folds, and makes great havock. When this happens, all the young men of the Kraal go in pursuit of him, directed by small dogs, who follow his scent: as soon as they discover him in the bushes, they irritate him, till he springs out with fury and attacks them; when being all armed with hassagays, they often throw twenty or thirty into his body at once; but it is common to lose a man or two in such attacks. These Hottentots were all cloathed in *crosses*, or mantles, made of the hides of oxen, which they dress in a particular manner, making them as pliant as a piece of cloth: they wore the hairy side outwards. Their breast, belly, and thighs, were naked, except being crossed by a number of leathern straps round their middle. They had no other covering for their private parts, than a muzzle of leather exactly covering the extremity of the *penis*, and suspended by a leathern thong from their girdle, which was commonly ornamented with brass rings. Some had the skin of a steenbock hung over their breast, with the skin of its fore legs and hoofs behind, which they look upon as a great ornament; others had a buffalo's tail, fastened to a girdle which was tied round the thigh; others a porcupine's quill stuck through each ear; others had plates of brass of six inches square fastened to their hair, hanging on each side of their head; others large ivory rings round their arms, with several other ridiculous fancies too tedious to mention. The women were dressed almost in the same taste, except that a great number of small thongs of leather, suspended from.

from their girdle, reached down to their knees, and in some measure concealed their nakedness. They have captains or chiefs over each Kraal, who claim the greatest part of the herds; the others seem only to be servants, though they have every thing in common, and pay little respect to their superiors. These Hottentots are called Gunaguas, but were mixed with another people whom the Dutch call Caffers, who border upon Terra de Natal. They were all armed with hassaguays, of which every one had eight or ten in his left hand. We found here the true Cape jassmine, or *gardenia stellata*, and the coral tree, *erethrina corallodendron*. The climate here differs much from that of the Cape. They have no S.E. wind, which is so troublesome there; their strongest wind is from the S.W. They seldom have rain in summer, though often thunder and lightning; the clouds being attracted by the lofty mountains are spent in showers before they reach the plain.

13th, 14th, We made but very short stages, employing our time in collecting plants, all of which were new. The buffalo is numerous in this country: it is a fierce animal, and larger than the biggest of our English oxen. In the day-time they retire to the woods, which renders it very dangerous to botanize there. We here saw two lions for the first time, at about 4 or 500 yards distance; but they took no notice of us, keeping their eyes upon a clump of the *capra dorcas*, which were feeding at some distance from them. We shot two of the buffaloes which proved good eating.

15th, To Zwart Kop's Rivier, where we rested all night.

16th, To Zwart Kop's Salt-pan, where we remained most part of that day. This Salt-pan is a lake several miles distant from the sea, and upon an eminence. In the rainy season it is filled with fresh water, which, by the saltness of the ground, soon becomes strongly impregnated with saline particles; and when the summer's heat exhales the fresh water, the bottom of the lake is covered with a crust of pure salt two or three feet thick. The lake is about three miles round, and surrounded by a rising ground, covered with a great variety of curious shrubs, many of which proved new. Here we found several singular insects, and among many others the *gryllus* and *cimex*.

17th, We travelled through a miserable parched country, covered with shrubs and succulent plants of various kinds; but the grass was entirely burnt up by the heat of the Sun. We saw numbers of wild animals, and in particular a variety of the Zebra, called by the Hottentots Opeagha. We also observed the print and dung of elephants and lions. At noon we came to Sunday's River, where we rested a few hours, and consulted with our guide, whom we took from the last Dutch place, about proceeding on our journey. But both he and our servants refused to advance further; telling us, we were now on the borders of a powerful nation of Hottentots, called Caffers; who, they said, would kill us, were it only to get the iron belonging to our waggons. In consequence of these remonstrances, and the bad state our carriages

were in, being ready to drop to pieces, and many of our oxen sick, we, with much reluctance, consented to return the same way we came.

20th, We arrived again at Sea-cow River, where we repaired our waggons.

24th, 28th, We proceeded homewards through Kromme Rivier and part of Lange Kloof; but being informed there was a hot bath about a day's journey to the Northward, we determined to see it, leaving our waggons and servants in Lange Kloof.

29th, Towards the evening we crossed the ridge of mountains on the North-side of Lange Kloof, and at night came to a solitary cottage belonging to a Dutchman, where we found several Dutch people, who were going next day to the hot bath, to use the water. We were glad of their company, and travelled over the driest country I ever beheld. The plains were covered with loose stones, and not a blade of grass to be seen; but we found many rare species of *crassula*, *mesembryanthemum*, and other succulent plants. In some places not a drop of water was to be found within thirty miles circuit. We could of course expect to see but few animals; those were the *capra dorcas*, *equus zebra*, *koodoes*, and springbucks.

30th, At night we arrived at the hot bath, which is situated at the foot of a ridge of dry mountains: the water is very hot, and tastes strongly of iron. There is a Dutch settlement about 300 yards from the fountain, where they float their gardens every night with

the water, which at that distance is still smoaking. By this means they have all kind of garden vegetables in the greatest perfection. Next morning we went up to the top of this ridge of mountains, which appeared like a mass of rocks heaped one on top of another, where we had an extensive view of the country, which appeared horrible, every thing being parched up, and even the beds of the largest rivers entirely dry. We found here a species of heath remarkable for having its branches and leaves all covered with a fine hoary down or nap, which we thought singular in that *genus*: we called it *erica tomentosa*.

Jan. 1st, We returned to Lange Kloof, and next day overtook our waggons; but many of our oxen were sick, having caught a disease which rages there amongst the horned cattle in summer, and so affects their hoofs that they often drop off, and great numbers die. This disease proves detrimental to the Dutch peasants, who live 5 or 600 miles in the country, when they make a journey to the Cape. Their oxen are often seized with it in the middle of a desert, and sometimes must remain there for a month till they recover. This makes their journeys to the Cape long and disagreeable, especially as they are obliged to take with them their wives and children, for fear of their being murdered by the Hottentots in their absence.

3d, We came to Great Thorney River, where we again parted with our waggons, in order to examine a large tract of Carro, where it was improper to take our

oxen on account of the scarcity of water. Late in the afternoon we came to a peasant's house, who informed us, he had a neighbour about four hours ride from his place, by whom we should be kindly received, and who would further direct us on our journey. After having put us in the road, and given us some directions, he parted with us, and we pursued our journey till sun-set, but found no habitation. We therefore concluded, that we had certainly lost our way, and returned some miles back, where we found a road which branched off another way. In this path we continued till one o'clock in the morning, having got into a dismal valley, inclosed on each side with rugged precipices: at last we found ourselves in the middle of a thicket of thorn trees (*mimosa nilotica*) where we unsaddled our horses and kindled a fire. We passed the night with little comfort, having eaten nothing all that day; but to our great satisfaction we heard the murmuring of a stream, which we went in search of, and found good water: our concern, however, was still great for our poor horses that had nothing to eat. We spent the night in gathering wood and keeping our fire up till day-light, when I climbed up a high precipice, and viewed the country. Here I collected several curious plants, *geranium spinosum*, *stapelia euphorbioides*; and ~~upon my~~ return, we mounted our horses, and directed our course towards the high mountains, where we expected to find some relief, but were disappointed; for after being parched up with insupportable heat, we met not with a drop of water to quench our thirst

during the whole day's journey. But towards the evening we happily discovered a house, where we were kindly entertained, and the next morning overtook our waggons in Hartwig's Kloof; but our oxen were in a bad state, and one of them was quite unfit for service. We continued our journey without any other remarkable event, except that of losing more of our oxen by the above mentioned disease.

12th, Came to Buffels Tagt River, where we rested several days, ranging the adjacent woods, where we found many curious trees in bloom.

29th, We arrived at the Cape Town, after a journey of four months and fourteen days.

T H I R D J O U R N E Y.

R. Feb. 29, 1776. **S**EP. 26, 1774, I set out from the Cape Town, but by the badness of the weather was obliged to stay all night at the Salt River, about two miles from the town, where there is a wine-house. I had in company only two servants, for driving my waggon and taking care of my oxen and horse.

27th, The morning being fine we travelled through the great sandy plain (lying between the Cape Town and Hottentots Holland Mountains) great part of which was under water. In the afternoon we had heavy rain, when

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we crossed the Eerste Rivier, and lodged all night at a farm-house under those mountains, where we found the whole country enamelled with flowers.

28th, 30th, The weather began to grow more pleasant, the Sun shining out with force; but sudden heavy showers much retarded our journey, confining us to short stages along the foot of the Stellebosch Mountains.

Oct. 1st, To Draaken Steen.

2d, To Paarle Kerk, where I was joined by Dr. THUNBERG.

4th, We went up to the top of the Paarle Mountain, where we added greatly to our collection.

5th, To Paarde Berg (Horse Mountain).

6th, We mounted to the top of Paarde Berg, where we found a treasure of new plants, which we had not seen before, and on the top had an extensive view of the adjacent country, which is level, and has but a barren appearance; yet contains several rich plantations, producing abundance of corn and wine; and the peasants live luxuriously. Their plantations lie all around the foot of this mountain, which yields a number of fine rivulets, without which this country would be uninhabited.

7th, We directed our course Northward, through a level country covered with low shrubs; but it being now spring, it was every where decorated with flowers of the greatest beauty, every hour's march producing new charms. At night we arrived at the foot of a mountain called Van Riebeck's Castle. There we lodged at Mr. DRAYER's, a wealthy

wealthy farmer, who treated us in the most friendly manner, and begged that we would favour him with our company for a month, which should not cost us a farthing.

9th, We went up to the top of Riebeck's Castle, which is very high, and on the North side inaccessible. It is about four or five miles long, and very narrow on the top; we collected here many remarkable new plants, in particular a hyacinth, with flowers of a pale gold colour.

10th, We came to the Berg Rivier, which was then impassable by reason of the late rain.

12th, With some difficulty we transported, in a large boat, our waggon and baggage to the opposite side, and afterwards obliged our oxen to swim over. From thence we proceeded through a barren uninhabited country; consequently were obliged to content ourselves with the shelter of a large *leucodendron*, that protected us from the S.E. wind, which at this season sometimes blows cold.

13th, We arrived at the foot of a mountain called Piquet Berg, lying direct North from the Cape Town, being a particular place of observation of the Abbé DE LA CAILLE, when he measured a degree on the meridian in the year 1750. All around the mountain the soil is sandy, but furnished with a great variety of beautiful plants, especially *aspalathi*.

15th, We mounted the Piquet Berg, which is very high but easy of ascent. On the top are fine plains covered

vered with excellent verdure, which are of great service to the peasants, who send up their oxen during the summer season. We saw here several zebras and two colts, but they were very shy.

18th, We came to Verloore Valley, which begins on the N.W. side of the Piquet Berg. It is a narrow extent of marshy ground, inclosed by hills on each side, with a small river, frequented by a variety of water fowl, which afforded good sport. Towards the sea, the river increases in breadth, in many places upwards of a mile, and is very deep; there we saw hundreds of pelicans and wild geese, which kept the middle of the river; but we shot several wild ducks and water hens, which swam among the reeds along the side of it.

23d, We arrived at the mouth of the Verloore River, where it is discharged into the sea; but found the coast barren, consisting of sandy hills, so loose that our horses were sometimes up to their bellies, which made our journey very fatiguing.

23d, We left the shore on our left hand, and directed our course Northward towards the mouth of the Olyfant's Rivier. The heat became now great, which the whiteness of the sand still increased, and obliged us to travel late in the evening and early in the morning, resting in the middle of the day. It was also not a little fatiguing to travel here on horseback, the mole-casts being so deep that the horses fell up to their shoulders every six or seven minutes. This animal is by the Dutch called Landmoll, but differs so much from the European mole, that
it

it does not belong to the same class of animals, but is entirely new. It feeds upon the roots of *ixiæ*, *gladioli*, *antholyzæ*, and *irides*, often grows to the size of a rabbit, and by some is esteemed good eating. There is another species of the animal, called by the Dutch Bles-moll, which inhabits the hard ground; but seldom exceeds the size of the common European mole. This country is furnished with a great variety of elegant shrubs; viz. *enista*, *partia*, and *aspalathi*. At night we came to Lange Valley, where we took up our lodging in a desolate place, the inhabitants being all removed; for this is only their winter residence, when the water is fresh, which had now began to be brackish.

24th, We set out early in the morning, expecting to find a river or fountain, where we could rest during the heat of the day; but, to our no small disappointment, we travelled till noon without finding any: our oxen were so hot that their tongues hung out of their mouths. About one o'clock we saw a lake of water at some distance, but on our arrival our horses refused to drink: we dismounted, and found it to be a salt lake. In the evening we came to a fountain of excellent water, where we spent the night with great comfort. Next morning we were visited by a peasant going to the Cape; who told us, he had been attacked in the night by a lion, which made a spring at his Hottentot who led the oxen, but happily missed him. He admonished us to be expeditious, and get to some habitation that night, otherwise we might expect a visit from him.

25th, At noon we proceeded on our journey, the road continuing still very bad; and in passing along we saw the prints of the lion's feet in several places. At night we came to Olyfant's Rivier, where we found a Dutch habitation; there we rested several days, being treated with great hospitality. This country abounds with game. They have two kinds of partridges, which are exceedingly plentiful and easy to shoot; and a person cannot walk ten paces without raising a brace of quails. Their hares are of an extraordinary size, but differ little otherwise in character from those of Europe. We hunted every day, and by the assistance of the peasant's son, who was an excellent marksman, never failed to come home laden. The sterile appearance of this country exceeds all imagination: wherever one casts his eyes, he sees nothing but naked hills, without a blade of grass, only small succulent plants. The soil is a red binding loam, intermixed with a kind of rotten *schistus* or slate. Next morning we traversed the adjacent hills, and were surprised to find all the plants entirely new to us. They were the greatest part of the succulent kind; viz. *mesembryanthemum*, *euphorbia*, and *stapelia*, of which we found many new species. The peasant told us, that in winter the hills were painted with all kind of colours; and said, it grieved him often, that no person of knowledge in botany had ever had an opportunity of seeing his country in the flowery season. We expressed great surprise at seeing such large flocks of sheep as he was possessed of subsist in such a desert; on which he observed,

7

that

that their sheep never ate any grafs, only fucculent plants, and all forts of shrubs; many of which were aromatic, and gave their flesh an excellent flavour. Next day I paffed through a large flock of sheep, where I faw them devouring the juicy leaves of *mesembryanthemum*, *stapelia*, *cotyledon*, and even the green feed veffels of *euphorbia*; by eating fuch plants they require little water, efpecially in winter.

30th, We were employed in unloading our waggons, and tranfporting our baggage acrofs the river in a fmall boat; and afterwards drove over our oxen with the empty waggons, which were almoft overfet in the middle of it. The river is about forty or fifty yards broad, and in fome places very deep. The borders are covered with the *mimosa nilotica*, which forms a thick impenetrable wood. We were about a day's journey from the mouth of this river, where are ftill fome elephants remaining, the country being very wild and uninhabited. We had now the great Carro to pafs; a defart of three days journey, where no fresh water, and only three pits of brackish water, enough to preferve the lives of our cattle, were to be found. Thefe pits are at fome diftance from the road, which makes it very difficult for ftrangers to find them. But while we were ferioufly confidering thefe approaching difficulties, thinking, if we fhould mifs the pits, we fhould probably perifh in this inhospitable defart, to our great joy we were overtaken by a Boor, with his wife and children, who were going the fame road; but he having a fresh team of horfes, we could not keep up with him.

However, he directed us in the way; and told us, he would tie a piece of white cloth on a branch of a tree, where he knew there was water; but desired us not to go to those places without fire-arms, as there was commonly a lion lurking near them; who knowing that all the animals must come there to drink, he seldom failed to seize his prey. At night we overtook our fellow traveller, who had taken up his lodging on a bare eminence, without a bush to shelter him; though at some distance there was a small wood of *mimosa* trees along the banks of a river that was then dry, which we thought much preferable to his situation. But he told us, it was much more dangerous on account of wild beasts; and that there often fell such sudden showers in the mountains, that people who had lodged by the rivers, had, with their waggon and oxen, been carried away in the night while they lay asleep. He left us early next morning, but we were obliged to stay till noon to let our oxen feed, and then went on until sun-set; but unhappily found no water, which mortified us much, having a long day's journey to the next watering place. All next day we travelled over this thirsty land, where we suffered from the heat of the Sun and want of water; but our sufferings were still aggravated when we thought on ~~our~~ poor animals, who often lay down in the yoke during the heat of the day. This desert is extensive; being bounded, on the N. and N.E. by a chain of flat mountains, called Bockland's Bergen (Bockland's Mountains) and on the W. and N.W. by the Atlantic Ocean.

It is uninhabitable in summer; but in winter, or during the rainy season, the Bockland people come down with their herds, which by feeding upon succulent shrubs, that are very salt, in a short time grow remarkably fat. There still remains a great treasure of new plants in this country, especially of the succulent kind, which cannot be preserved but by having good figures and descriptions of them made on the spot; which might be easily accomplished in the rainy season, when there is plenty of fresh water every where. But at this season of the year, we were obliged to make the greatest expedition to save the lives of our cattle, only collecting what we found growing along the road side, which amounted to above 100 plants, never before described. Towards the evening we arrived at the foot of Bockland's Berg, where we passed the night by a penurious stream of fresh water, but which yielded us no small comfort.

Nov. 2d, The peasant who had passed us in the Carro, as soon as he arrived at home, immediately sent two team of fresh oxen to help us up the mountain, our own being much weakened by the heavy roads. In the cool of the afternoon we ascended by a winding road, which was so very rugged and steep, that it took five hottentots with ropes made fast to the waggon to keep it from overturning. The face of the mountain consists intirely of scattered rock, being accessible only in this place, and is overgrown with a great variety of large woody plants, most of which were new. We found a new species of aloe here, called by the Dutch Koker Boom,

Boom, of which the Hottentots make quivers to hold their arrows; it being of a soft fibrous consistence, which they can easily cut out, leaving only the bark, which is hard and durable. These trees were about twelve feet high, with a straight smooth trunk, about ten inches or a foot diameter and five or six feet in length, which divided into two branches; and those were again sub-divided into two more branches, which terminated in a bunch of thick succulent leaves surrounding the stem, spear-shaped, entire, without spines, and hanging down like the leaves of *dracena draco*. We did not see it in flower, but by the above characters took it for a new species, and called it *aloe dichotoma*. We gained the top of the mountain, and entered into Bockland, which is extended along the summit for many miles. It is pretty level, but very rocky. We enjoyed a pure cool air, it being several degrees colder here than in the Carro. Bockland lies nearly in a Northern direction from the Cape, and at the distance of about 220 miles. It was called Bockland on account of the amazing quantity of spring bucks which were formerly found there; but since this country has been inhabited by Europeans, it has ceased to be the settled residence; at least, the number of those which constantly remain in it is very inconsiderable. It generally happens, however, once in seven or eight years, that flocks of many hundred thousands come out of the interior parts of Africa, spreading over the whole country, and not leaving a blade of grass or a shrub. The peasants are then obliged to guard their corn fields night and day, otherwise those animals would cause a famine wherever they passed.

passed. It seems probable, by the accounts of these extraordinary emigrations, that their natural habitation is in the interior parts of Terra de Natal; and that they are forced Southwards by dry seasons, which happen sometimes in those regions to such a degree, that not a drop of rain will fall for two or three years together. These great flocks are said to be always attended by lions; and it is observed, where a lion is, there is a large open space. We saw several flocks, but not exceeding twenty in each. We met a party of Dutchmen, who had been about 150 miles to the Northward of Bockland, destroying the Boschman Hottentots. They informed us, they had seen great flocks of the spring bucks; but there happening much rain, which had recovered the grass and vegetation, they had been observed to change their course, and return to the interior parts of the country.

3d, 4th, We continued our journey along this elevated tract; having on our right hand, or South-side, the precipice, which is inaccessible; and on the North-side, a desolate hilly country, inhabited by a few wandering tribes of the Boschman Hottentots. At night we came to the place of our benefactor, whose name was KLAAS LOSPER; he was a very opulent man in those parts, having upwards of 12000 sheep and 3000 bullocks. Most of the plants that we collected here were new; and, I believe, many more remain, this having been the dry season, when most of the flowers were gone.

6th, We directed our course Northward, through a dry, barren country, called Hantum; and on the 10th came

came to the last Dutch habitation on this side of the country. As we passed along we found many new plants growing near the banks of rivers, which were then quite dry; but the soil consisted of nothing but rotten rock. The hills were of the same substance, all of a conical figure, and entirely covered with pieces of rock, about the size of a man's fist. We continued several days at this habitation, where we were well entertained. They had excellent bread, good mutton, butter and milk, but no kind of strong liquors. We made several enquiries about the country lying to the Northward; and were told, that it had been formerly inhabited by Europeans near a hundred miles further, who at first had greatly increased their herds; but that some dry seasons coming on afterwards, they had been forced to return: the country therefore was supposed to be uninhabited, except by the wandering Hottentots, who seldom stay above a month in a place. This place is about 350 English miles North from the Cape of Good Hope. We now changed our course, going directly S.E. through an uninhabited country much like the former, surrounded by high mountains, flat on the tops, and forming what the peasants call Table Mountains. I never saw the smallest rivulet or fountain issuing from them; all the water that we found being that which was left stagnant in the deepest parts of the rivers, that are formed by the rain in the winter season, which rivers, towards Midsummer, in other places become entirely dry.

14th, To ~~Rhinoceros~~ Rivier. Here we saw great herds of zebras, and were informed by three Dutchmen, who passed us on horseback, that this place was frequented

quented by a large lion; and, as a proof, they shewed us a zebra, which he had lately killed; assuring us, if we stayed all night there, he would pay us a visit. We travelled about ten miles further, and at night saw a flock of sheep and some bullocks, which greatly animated us, expecting to find some habitation where we might shelter ourselves during the night; but, when we came to the place where the sheep were, we found a Dutchman with his wife and several young children sitting under the shelter of some bushes, which they had formed into an alcove, to screen them from the heat of the Sun. We stayed here all night, and the man asked us to sup with them; which we did, and made them a present of some tea and tobacco, which they thankfully received; and the next day the husband saddled his horse, rode six or seven miles with us, and gave us very good directions how to proceed in our intended course.

16th, We ascended a flat chain of mountains, called Rogge Velds Berg, where we found the road extremely rugged. Rogge Veld extends along the summit of a high ridge of mountains, running obliquely across the country for several hundred miles. It is very arid, except in some vallies, where the Dutch peasants have their habitations; but the general face of the country is rock. The soil is a red ochrey loam; it binds very hard in summer, and is in most places salt, which causes bad water. There is not a tree in the whole country, unless we should so call a few miserable shrubs, and of these the largest not

exceeding two feet in height. The air is very sharp, and in winter they have frost and snow for several months, which obliges the Boors to remove, with all their flocks and herds, down to the Carro, or lower defarts, where they spend the winter; and at that time have plenty of fresh water, and all the shrubs green, which afford food for their cattle. They remove down in the beginning of May, when they have sown their corn, and return about the latter end of October, when the low country becomes parched, and the water turns salt, or is entirely dried up. All the game and ferocious animals observe the same removes. The ancient inhabitants of this country, called by the Dutch Boschmenschen, are a savage people and very thievish; often carrying off 700 sheep at a time, and killing their shepherds. They use bows and arrows, and poison the arrows with the venom of serpents mixed with the juice of a species of *euphorbia*, which we had no opportunity of seeing. These Hottentots have neither flocks or herds, nor any fixed habitation, nor even skins to cover them; but live in the cavities of rocks, like baboons. Their common food is roots of plants, many of which we have not been able to discover. They eat snakes, lizards, scorpions, and all kind of reptiles. There is a caterpillar which produces a very large moth, and is found commonly on the ~~*minifavosita*~~ *minifavosita*. These are found in great plenty, often stripping the tops of all their leaves, and of them the Hottentots make ~~many~~ a delicious meal. They also eat the
 I eggs

eggs of a large species of ant, which they dig out of the ground in great quantities, washing them in water, and afterwards boiling them. They are commonly called Hottentot's rice. This is an excellent country for sheep; but the inhabitants breed few oxen, and those only for their own use. We found few plants here; but those we found were all new. I did not see an *erica* or *protea* in the whole country.

22d, The ground was white with frost, and the wind sharp. At first we proposed to continue our journey along the top of these mountains to the N.E. extremity; but our waggons were so shaken by the ruggedness of the road, and our horses and oxen so tender-footed, that they became unserviceable, and we were obliged to drive them loose a great part of the way home.

Dec. 2d, We thought of descending the mountain, and directing our course to the Cape; but it blew a violent storm, and was extremely cold. The next morning the ground was white with frost, and there was ice upon the pools as thick as a crown piece. This alarmed the peasants, their wheat being then in blossom, which they expected would be entirely destroyed: a circumstance that often happens in this country.

3d, We were furnished with fresh oxen, and several Hottentots, who, with long thongs of leather fixed to the upper part of our waggons, kept them from overturning, while we were obliged to make both the hind

wheels fast with an iron chain to retard their motion. After two hours and a half employed in hard labour, sometimes pulling on one side, sometimes on the other, and sometimes all obliged to hang on with our whole strength behind the waggon, to keep it from running over the oxen, we arrived at the foot of the mountain, where we found the heat more troublesome than the cold had been on the top. We now entered a large division of the Carro which lies along the foot of the Rogge Veld's Mountains, being a defart of four days journey, with no more than three pits of brackish water to be found in all that extent, which was at this season forsaken by every living creature; but in winter it is the habitation of the Rogge Veld Boors, as I observed before.

5th, To Unlucky River, called so from a man having been there formerly devoured by a lion. We remained here a day to rest our oxen, having found a pit with brackish water, and some reeds, which the oxen devoured with greediness.

8th, About eleven o'clock at night we got clear of the defart, and arrived at the foot of the Boeke Velde mountains, where we lodged by a rivulet of pure fresh water; and we spent the remainder of that night and part of next day in great luxury.

11th, To Verkeerde Valley, where we rested three days, having found good pasture for our oxen, and a large lake of fresh water, well stocked with water-fowl. We
lived

lived on wild ducks and snipes, though the fields abounded also with korhaans (a kind of bustard), partridges, hares, &c. and great flocks of ostriches.

15th, To Hexen Rivier, which runs along a narrow passage through the great chain of mountains, between Rood Land and Zwellendam. This valley is inclosed on each side with impassable mountains, whose tops were still covered with snow. There are several very genteel habitations in it, where we got some wine and excellent fruit. We found many rare plants on the sides of these lofty mountains; and, I believe, there still remain many more entirely unknown to us.

18th, To Breede Rivier (Broad River).

22d, To Rood Land.

26th, To Paarde Berg.

28th, To the Cape Town.

METEOROLOGICAL JOURNAL

KEPT AT THE HOUSE OF

THE ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January 1775.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Jan. 1	8	0	34,0	35,5	29,75		NW	1	Fine.
	2	0	41,0	37,0	29,71		NW	1	Fair.
2	8	0	29,0	34,5	30,17	0,062	W	1	Fine.
	2	0	33,0	34,0	30,19		W by N	1	Fine.
3	8	0	39,5	36,5	30,02	0,114	W by N	0	Fog.
	2	0	43,5	37,5	29,91		WNW	1	Fine.
4	8	0	39,5	40,5	29,82	0,016	W	2	Fair.
	2	0	44,0	41,5	29,87		W	1	Fine.
5	8	20	40,0	41,5	30,02		W	1	Fair.
	2	0	45,0	43,0	29,98		WNW	1	Fair.
6	8	0	45,5	44,5	29,93		WNW	1	Fine.
	2	30	48,0	46,0	30,06		WNW	1	Fine.
7	8	0	46,0	45,0	30,06		WNW	1	Fair.
	2	0	50,5	47,0	30,00		WNW	1	Fine.
8	8	0	52,0	49,0	29,92		WNW	2	Cloudy.
	2	0	54,0	50,0	29,91		WNW	1	Fair.
9	8	0	48,5	51,0	30,18	0,034	WNW	1	Cloudy.
	2	10	52,5	52,0	30,21		WNW	1	Fair.
10	8	0	48,5	51,5	30,18	0,010	WNW	1	Cloudy.
	2	0	49,5	51,5	30,18		WNW	1	Fair.
11	8	0	47,0	50,0	29,86		WNW	2	Rain.
	2	0	48,5	50,0	29,73		WNW	2	Rain.
12	8	0	41,0	48,5	29,69	0,086	WNW	1	Fine.
	2	0	47,0	49,5	29,69			1	Fine.
13	8	0	40,5	45,5	29,74	0,030		1	Fine.
	2	30	45,5	47,0	29,75			1	Fine.
14	8	30	45,5	45,0	29,75			1	Cloudy.
	2	10	49,0	46,5	29,72			1	Rain.
15	8	30	46,0	47,5	29,77	0,115		1	Rain.
	2	0	47,0	49,0	29,81			1	Cloudy.
16	8	0	41,5	45,5	29,83			2	Fine.
	2	0	47,0	47,0	29,85			1	Fine.

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	Time.		Therm.	Therm.	Barom.	Rain	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Jan. 17	8	0	42,0	45,5	29,84		ESE	1	Fine.
	2	0	48,5	47,0	29,76		E	1	Fine.
18	8	0	34,0	43,5	30,03	0,055	NNW	1	Fog.
	2	0	37,5	43,5	30,04			1	Fog.
19	8	0	35,0	39,5	29,99		NE	1	Fog.
	2	0	36,0	40,0	29,94		NE	1	Fair.
20	8	0	34,0	38,5	29,88		E	1	Fog.
	2	0	33,0	39,0	29,82		ENE	1	Snow.
21	8	0	38,5	39,0	29,66	0,230	ENE	1	Fog.
	2	0	47,5	44,0	29,59		S by E	1	Fair.
22	8	0	42,0	42,5	29,51		SSE	1	Fine.
	2	0	49,5	45,0	29,47		S by E	1	Cloudy.
23	8	0	44,5	45,5	29,54	0,140	E by N	1	Rain.
	2	0	44,5	46,5	29,63		ENE	1	Fair.
24	8	0	32,0	41,5	30,13	0,204	NE	2	Cloudy.
	2	0	32,0	40,0	30,25		E by N	1	Fine.
25	8	0	25,5	33,5	30,38		E	1	Fine.
	2	15	26,5	34,0	30,28,5		ESE	1	Fair.
26	8	0	26,5	30,5	29,71		E	1	Snow.
	2	0	40,5	33,5	29,60		WSW	2	Fair.
27	8	0	38,5	36,0	29,78	0,098	SE	1	Fair.
	2	0	46,5	39,0	29,62		SSE	2	Fair.
28	8	0	36,0	40,5	29,62	0,127	SW	1	Fair.
	2	10	45,5	42,5	29,70		WSW	1	Fine.
29	8	30	49,0	43,5	29,46,5	0,323	SSW	1	Fair.
	2	0	52,5	46,0	29,46,5		SW	1	Fair.
30	8	0	45,0	47,5	29,55		SW	1	Fine.
	2	0	49,5	48,5	29,63		SW	1	Fine.
31	8	0	50,5	49,0	29,32	0,080	SSW	3	Rain.
	2	0	53,5	50,0	29,30		SSW	2	Fair.

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for February 1775.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
						Points	Str	
	H. M.			Inches.	Inch.			
Feb. 1	8 0	51,5	50,5	29,14	0,097	SSW	2	Rain.
	2 10	54,0	52,0	28,99	.	SSW	2	Fair.
2	8 0	43,0	48,5	29,52	0,167	SW	2	Fair.
	2 0	47,0	49,5	29,50		SW	2	Rain.
3	8 0	44,5	48,5	29,60	0,165	SSW	1	Rain.
	2 0	52,0	50,0	29,54		SSW	1	Rain.
4	8 0	53,0	51,5	29,565	0,034	WSW	2	Cloudy.
	2 0	55,0	52,5	29,45		WSW	2	Fair.
5	8 0	39,0	49,0	29,89	0,199	N	1	Fine, windy night.
	2 0	41,0	48,0	30,03		WNW	1	Fine.
6	8 0	39,0	44,0	30,26		SE	1	Cloudy.
	2 0	46,0	45,0	30,24		SE	1	Fair.
7	8 0	48,5	46,5	30,02	0,030	WSW	1	Fair, windy night.
	2 0	53,5	49,0	29,92		SW	2	Fair.
8	8 0	51,0	51,0	29,60	0,049	SW	2	Rain, windy night.
9	8 0	41,0	48,0	29,35	0,225	SW	2	Fair, windy night.
	2 0	48,0	49,0	29,35		SW	1	Fair.
10	8 0	37,0	45,0	29,75	0,165	W	1	Fine.
	2 0	48,5	47,0	29,65		W	1	Fine.
11	8 0	42,0	46,5	29,02	0,092	SW	2	Cloudy, windy night.
	2 0	51,0	47,5	28,98		SW	1	Fair.
12	7 45	37,5	44,5	28,94	0,268	SW	1	Fine.
	2 0	43,5	46,0	28,91		SW	1	Fair.
13	7 45	41,0	44,5	28,89	0,095	SW	1	Fair.
	2 10	46,5	45,5	28,935		SW	1	Fair.
14	8 0	40,0	43,5	29,35	0,019	WNW	1	Fair.
	2 0	45,5	45,0	29,50		WNW	2	Fair.
15	8 0	42,0	43,5	29,225	0,221	WSW	1	Cloudy.
	2 0	48,5	45,0	29,44		NNE	2	Fine.
16	8 0	36,5	42,5	29,75		S	1	Fair.
	2 0	47,0	44,5	29,55				

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for February 1775.

	Time.		Therm.	Therm.	Barom	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Feb. 17	8	0	38,0	44,5	29,70		WSW	1	Fair.
	2	0	45,5	45,0	29,83		WSW	1	Fine.
18	8	0	35,0	41,5	30,07		WSW	1	Fine.
	2	0	46,5	44,0	30,15		WNN*	1	Fine.
19	8	0	43,5	42,5	30,12	0,018	N by W	1	Fine.
	2	0	48,5	44,5	30,27		N by W	1	Fine.
20	8	0	44,5	43,5	30,37		SSW	1	Fair.
	2	0	53,0	46,5	30,34		SW	2	Fair.
21	8	0	47,5	47,0	30,18	0,010	SW	2	Rain.
	2	0	49,5	49,0	30,17		NNW	2	Rain.
22	8	0	35,5	43,5	30,48	0,016	SSW	1	Fine.
	2	0	48,5	46 0	30,48		WNW	1	Fine.
23	8	0	40,0	43,5	30,30		SSW	1	Fair.
	2	30	48,5	45,5	30,19		SW	1	Fair.
24	8	0	49 0	47,5	30,11	0,058	SW	1	Cloudy.
	2	0	51,5	49,5	30,11		SW	1	Fair.
25	8	0	40,0	47,5	30,04		S	1	Fine.
	2	0	50,0	49,0	30,00		SSW	1	Fine.
26	8	0	37,5	45,5	30,08		NE	1	Fair.
	2	0	52,0	47,5	30,08		SSE	1	Fine.
27	7	45	35,0	44,5	30,14		NNE	0	Fair.
	2	0	53,5	46,5	30,14		SW	1	Fine.
28	8	0	39,0	45,0	30,09		S	1	Fair.
	2	0	54,0	47,0	30,07		SSW	1	Fine.

* So it is in the written journal, probably for WNW. s. HORSLEY.

METEOROLOGICAL JOURNAL, &c.

F O R O N E Y E A R,

BEGINNING WITH THE MONTH OF MARCH, 1775.

A Committee of the Society lately appointed to regulate certain matters relating to Meteorology have determined, that the Society's Meteorological Year shall, from this time forward, commence with the month of March; to the end, that every such year may consist of one entire summer and one entire winter, rather than of the pieces of two different winters with the entire summer intervening, as must always be the case when the Meteorological Year, in conformity to the civil reckoning, begins with the month of January.

S. HORSLEY.

METEOROLOGICAL JOURNAL.

for 1775.

	Time.		Therm.	Therm	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Mar. 1	7	0	38,5	46,0	30,00		SSW	1	Fair.
	2	0	56,0	49,5	29,93		S	1	Fine.
2	7	0	43,5	48,5	29,80	0,190	SW	0	Fair.
	2	0	50,5	50,5	29,75		W by S	1	Fine.
3	7	0	39,0	46,5	29,72		SW	1	Fair.
	2	0	44,0	48,0	29,76		W	1	Rain.
4	7	0	38,5	44,0	29,34	0,074	SE	1	Rain.
	2	0	47,0	46,5	29,07		S	1	Rain.
5	7	0	41,0	45,0	29,07	0,095	NE	1	Fair.
	2	0	43,0	40,5	29,23		N	1	Rain.
6	7	0	38,0	44,5	29,54	0,295	SSW	1	Fair.
	2	0	51,0	47,0	29,53		SSW	1	Fair.
7	7	0	38,0	45,5	29,66	0,010	SW	1	Fair.
	2	0	49,0	47,5	29,66		SSW	1	Fine.
8	7	0	35,5	44,0	29,60	0,087	S	1	Fine.
	2	0	46,0	46,0	29,47		WSW	1	Fair.
9	7	0	46,0	44,5	29,75		S by W	1	Cloudy.
	2	0	54,0	47,5	29,70		S by E	2	Fine.
10	7	0	42,0	48,0	29,61	0,169	SW	1	Fair.
	2	0	50,5	49,5	29,71		W	1	Fair.
11	7	0	43,5	47,5	29,865	0,046	SSW	2	Rain.
	2	0	47,5	48,5	29,57		SSW	2	Rain.
12	7	0	46,5	48,5	29,40	0,294	W by S	2	Fair.
	2	0	50,5	50,5	29,43		W	2	Fine.
13	7	0	39,5	46,5	30,02	0,053	W	1	Fair.
	2	0	47,0	48,0	30,27		NE	2	Fair.
14	7	0	33,0	43,0	30,60		SSW	1	Fine.
	2	0	46,0	44,5	30,61		W by S	1	Fine.
15	7	0	33,0	42,0	30,61		WSW	1	Fine.
	2	10	50,0	44,0	30,58		SW	1	Fine.
16	7	0	33,0	42,0	30,52		SSW	1	Fine.
	2	0	49,0	44,5	30,44		NNW	1	Fine.

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	Time.		Therm.	Therm.	Baom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Mar. 17	7	0	35,0	43,0	30,45		SSW	1	Fine.
	2	0	52,0	46,0	30,24		SW	1	Fine.
							SW	1	Fair.
18	7	0	40,0	44,5	30,02		W by S	1	Fair.
	2	0	52,5	47,5	29,88		W by S	1	Fair.
19	7	0	39,5	46,5	29,35	0,189	N by W	1	Rain.
	2	0	45,5	48,0	29,42		NW	1	Fine.
20	7	0	36,0	44,5	29,78	0,065	W by S	1	Fair.
	2	0	51,0	47,0	29,79		NW	1	Fair.
21	7	0	47,0	48,5	29,85		NW	1	Fine.
	2	0	58,0	52,0	30,04		SW	1	Fair.
22	7	0	47,5	50,0	30,10		SW	1	Fine.
	2	0	56,5	53,0	30,05		NW	1	Fair.
23	7	0	46,0	50,5	30,06		N by W	1	Fine.
	2	0	54,5	51,5	30,13		WSW	1	Cloudy.
24	7	0	40,0	48,5	30,14		W	2	Fair.
	2	0	57,0	52,0	30,04		S by W	2	Cloudy.
25	7	0	48,5	50,5	29,80		S by W	2	Cloudy.
	2	0	54,0	52,5	29,68		WSW	2	Fine.
26	7	0	37,5	47,5	29,72	0,093	NNW	2	Rain.
	2	0	42,5	48,0	29,66		NW	2	Fine.
27	7	0	30,5	41,5	29,79		SSW	2	Fine.
	2	0	43,5	43,5	29,80	0,041	SW	2	Rain.
28	7	0	42,5	42,5	29,28	0,025	NW	2	Fair.
	2	0	38,0	43,0	29,31		W	1	Fine.
29	7	0	28,5	38,0	29,30	0,079	W by N	1	Fine.
	2	0	39,0	39,5	29,38		N	1	Fine.
30	7	0	30,0	37,5	29,61	0,022	NNW	1	Fine.
	2	0	42,0	40,5	29,70		NW	1	Fine.
31	7	0	29,0	37,0	29,81	0,027	SW	1	Fine.
	2	0	47,0	42,5	29,81				

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Apr. 1.	7	0	36,5	40,0	29,94		SW	1	Fair.
	3	0	48,0	43,0	30,07		NE	1	Fair.
	2	7 20	37,0	42,0	30,10		SW	0	Fair.
2	2	0	55,0	45,0	30,18		WSW	1	Fine.
	3	7 0	38,5	44,5	30,18		WSW	0	Fine.
	2	0	54,5	49,5	30,13		SW	1	Fine.
4	7	0	46,0	48,0	30,00		S by W	1	Fine.
	2	0	53,0	50,0	30,00		W	1	Rain.
	5	7 0	39,5	47,0	30,20		NW	1	Fine.
5	2	0	54,0	49,5	30,22		NNE	1	Fine.
	6	7 0	38,5	47,0	30,23		SSE	1	Fine.
	2	0	54,5	51,0	30,23		NE	1	Fair.
7	7	15	46,0	50,0	30,27		NNE	1	Fair.
	2	0	52,0	51,5	30,31		NE	2	Fair.
	8	7 0	45,0	49,0	30,36		NNE	2	Fair.
8	2	0	57,5	50,0	30,36		NNE	2	Fair.
	9	7 0	42,5	47,5	30,36		N	1	Cloudy.
	2	0	50,5	49,5	30,355		N	1	Fine.
10	7	0	43,0	47,0	30,28		N	1	Fair.
	2	0	50,0	49,5	30,24		N by E	1	Fair.
	11	7 0	45,5	48,0	30,16		SW by W	1	Cloudy.
11	2	0	57,5	52,0	30,13		W	1	Fair.
	12	12 0	45,0	49,0	30,20		NE	1	Cloudy.
	2	0	54,5	51,5	30,22		NE by E	1	Fair.
13	7	0	46,0	50,0	30,26		S	1	Fine.
	2	0	56,0	52,5	30,26		NE	1	Fine.
	14	7 0	43,0	49,5	30,21		S	1	Fine.
14	2	0	62,0	54,0	30,07		S	2	Fine.
	15	7 0	50,0	52,0	29,68		S by W	2	Cloudy.
	2	0	52,5	53,5	29,69		W by S	1	Fair.
16	7	15	47,0	50,5	29,50	0,407	SW	1	Rain.
	2	0	57,5	54,0	29,55		N by W	1	Fair.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Apr. 17	7	0	50,5	53,0	29,625	0,025	WNW	1	Fine.
	2	0	59,0	55,5	29,625		N by W	1	Fair.
18	7	0	47,0	53,0	29,585		W by S	1	Fine.
	2	0	56,5	54,5	29,59		W by S	1	Fine.
19	7	0	41,0	50,0	29,63	0,118	W by N	1	Fine.
	2	0	53,5	52,5	29,725		W by N	1	Fine.
20	7	0	40,0	48,5	29,98		SSW	1	Fine.
	2	0	59,0	51,5	30,02		S	1	Fine.
21	7	0	47,0	49,0	29,99		E	1	Fine.
	2	0	60,0	53,5	29,90		ESE	1	Fine.
22	7	0	50,0	52,0	29,61	0,016	S	0	Rain.
	2	0	59,0	55,5	29,59		SW	1	Fair.
23	7	0	45,5	53,0	29,71		SW	1	Fine.
	2	0	54,0	54,5	29,85		SW	1	Fair.
24	7	0	48,0	53,5	30,02	0,116	SSW	1	Rain.
	2	0	62,0	57,0	30,08		NW	1	Fair.
25	7	0	50,0	55,0	30,19		SW	1	Fair.
	2	0	60,0	57,5	30,20		SSW	1	Fair.
26	7	0	53,5	56,5	30,18		S	1	Fair.
	2	0	72,0	61,0	30,15		S	1	Fine.
27	7	0	52,0	55,5	30,09		ENE	1	Fine.
	2	0	73,0	63,5	30,05		ESE	1	Fine.
28	7	0	55,0	57,5	30,06		NE	1	Fine.
	2	0	79,0	65,0	30,06		SW	1	Fine.
29	7	0	62,0	62,5	30,02		SE	1	Fine.
	2	0	83,5	69,0	30,02		S by E	1	Fine.
30	7	0	62,0	65,5	30,00		SE	1	Fine.
	2	0	74,0	69,0	30,01	0,386	W	1	Fine.

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	Time.		Therm.	Therm	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
May 1	7	0	54,5	63,5	30,12		NW	1	Cloudy.
	2	0	62,0	63,0	30,24		N	1	Fair.
2	7	0	51,5	60,5	30,39		SSW	1	Fair.
	2	0	68,5	63,5	30,40		W	1	Fine.
3	7	0	53,5	60,5	30,32		SSW	1	Fine.
	2	0	69,0	64,0	30,23		NW	1	Fine.
4	7	0	50,0	57,5	30,06		NE	1	Fair.
	2	0	73,0	65,5	29,95		S by E	1	Fine.
5	7	0	57,0	62,0	29,68		WSW	1	Fine.
	2	0	68,0	64,5	29,74		SSW	1	Rain.
6	7	0	49,5	57,0	29,96	0,232	N	2	Fine.
	2	0	60,5	59,0	30,06		NW	1	Fine.
7	7	0	56,0	57,5	30,14		SSW	1	Fine.
	2	0	67,0	61,0	30,13		SSW	1	Fair.
8	7	0	55,5	59,0	30,00	0,039	W	1	Cloudy.
	2	0	63,5	61,5	30,02		W by S	2	Cloudy.
9	7	0	56,0	59,0	30,08		W by S	1	Cloudy.
	2	0	67,0	63,0	30,08		SSW	2	Fine.
10	7	0	52,5	60,0	30,05		N	2	Fine.
	2	0	61,0	60,5	30,09		NW	2	Fine.
11	11	0	49,0	53,0	30,19		N	1	Fine.
	2	0	57,0	57,0	30,18		N.	1	Fine.
12	7	0	48,5	55,0	30,05		W	0	Rain.
	2	0	51,5	56,5	30,02		W	1	Rain.
13	7	0	49,0	54,5	30,12	0,058	NNE	1	Fair.
	2	0	59,5	56,0	30,15		E by N	1	Fine.
14	7	0	48,0	54,5	30,26		NNE	1	Fine.
	2	0	64,5	57,0	30,26		NW	1	Fine.
15	7	0	48,5	58,5	30,17		W by S	1	Fair.
	2	0	66,0	61,5	30,11		N by W	1	Fine.
16	7	0	52,0	57,5	30,02		WNW	1	Fine.
	2	0	68,5	61,0	29,86		W by S	2	Fine.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
May 17	7	0	51,5	59,5	29,81		NW	1	Fine.
	2	0	61,0	60,5	29,81		NW	1	Fine.
18	7	0	47,0	54,0	30,06		N	1	Fair.
	2	0	55,0	56,0	30,12		NNE	1	Fine.
19	7	0	46,0	53,0	30,00		WSW	1	Rain.
	2	0	55,0	55,0	29,92		N	1	Rain.
20	7	0	43,0	50,0	30,03	0,171	N	1	Fine.
	2	0	52,5	52,5	30,12		N	1	Fair.
21	7	20	48,5	51,5	30,20		WSW	1	Rain.
	2	0	62,5	55,0	30,21		W by N	1	Fair.
22	7	0	51,5	55,0	30,24		NE	1	Fine.
	2	0	58,0	57,5	30,25		ENE	1	Fair.
23	7	0	50,0	55,0	30,18		NE	1	Cloudy.
	2	0	56,5	66,5	30,11		ENE	1	Fair.
24	7	0	48,0	54,5	29,99		ENE	1	Cloudy.
	2	0	65,0	58,0	29,885		SE	1	Fine.
25	7	0	52,0	57,0	29,83		NNW	1	Fair.
	2	0	55,0	58,0	29,88		N	1	Fair.
26	7	0	51,0	56,0	30,06		N	1	Fair.
	2	0	50,5	57,0	30,14		NE by N	1	Rain.
27	7	0	49,0	55,0	30,24	0,052	NW	1	Fair.
	2	0	64,0	58,0	30,27		NE by N	1	Fine.
28	7	0	55,5	57,5	30,34		W	1	Fair.
	2	0	67,0	60,5	30,37		N	1	Fair.
29	7	0	57,0	60,0	30,43		SW	1	Fine.
	2	0	68,5	63,5	30,41		WNW	1	Fair.
30	7	0	58,5	62,5	30,38		SE	1	Fine.
	2	0	70,0	64,5	30,37		SE	1	Fine.
31	7	0	59,0	62,5	30,34		SSW	1	Fair.
	2	0	74,5	67,0	30,31		NNE	1	Fine.

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	Time.		Therm without	Therm within.	Barom	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str	
June	1	7 0	57,0	63,0	30,30		ENE	1	Fine.
		2 0	69,0	66,0	30,29		E	1	Fine.
	2	7 0	52,0	60,5	30,27		NNE	2	Fair.
		2 0	68,5	63,0	30,19		NE	2	Fine.
	3	7 0	56,5	61,5	30,18		NE	1	Fine.
		2 0	71,0	64,0	30,15		NE	1	Fine.
	4	7 0	57,5	61,5	30,15		NE	1	Fine.
		2 0	73,0	65,0	30,15		E by N	1	Fine.
	5	7 0	62,0	63,5	30,12		E by N	1	Fine.
		2 0	76,0	67,5	30,065		ESE	2	Fine.
	6	7 30	67,0	67,5	29,985		E by N	1	Fine.
		2 0	81,5	73,5	29,92		E by N	1	Fine.
	7	7 0	66,0	69,5	29,79	0,010	NNE	1	Fair.
		2 0	79,0	73,0	29,76		E by S	1	Fair.
	8	7 0	66,5	71,0	29,78	0,010	ENE	1	Fine.
		2 0	73,5	73,0	29,86		S	1	Fine.
	9	7 0	62,0	68,0	29,98		E by N	1	Fine.
		2 0	77,0	72,0	29,92		SE	1	Fine.
	10	7 0	65,5	70,0	29,71		SSE	1	Cloudy.
		2 0	72,5	72,0	29,82		SE	1	Cloudy.
	11	7 0	56,0	67,0	29,85	0,078	NNE	1	Rain.
		2 0	71,0	69,5	29,85		ENE	1	Fair.
	12	7 0	61,5	67,0	29,845	0,122	SSE	0	Fair.
		2 0	74,0	70,0	29,91		SW	1	Fine.
	13	7 0	62,0	67,0	29,99		ESE	1	Fine.
		2 0	78,0	71,0	30,00		S	0	Fine.
	14	7 0	64,0	68,0	30,00		NE by E	0	Fine.
		2 0	78,0	71,5	29,97		E by N	1	Fine.
	15	7 0	59,0	67,5	30,04		NE	1	Fair.
		2 0	73,5	71,0	30,055		NW	1	Fine.
	16	7 0	59,5	67,5	30,055		N	1	Fine.
		2 0	73,5	69,0	29,99		N	1	Fine.

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	Time.		Therm.	Therm.	Barom	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
June 17	7	0	60,0	65,5	29,855		ENE	1	Fine.
	2	0	78,0	70 0	29,76		SSW	0	Fine.
18	7	0	58,0	66,0	29,78		NE	1	Fair.
	2	0	76,0	69,5	29,83		NNE	1	Fair.
19	7	0	59,0	65,5	30,02		NE	1	Fair.
	2	0	67,5	68,5	30,11		NNE	1	Fair.
20	7	0	62,0	65,0	30,04		SW	1	Fair.
	2	0	77,0	70,5	29,99		WSW	1	Fair.
21	7	0	61,0	66,0	30,11		NW	1	Fine.
	2	0	77 0	69,5	30,11		W	1	Fair.
22	7	0	65,0	69,0	30,065		SW	1	Fair.
	2	0	78,0	73,0	29,93		WSW	2	Fine.
23	7	0	65,5	69,5	29,93		WSW	2	Fair.
	2	0	72,0	72,0	29,86		WSW	2	Fair.
24	7	0	60,5	68,0	29,77		W	1	Cloudy.
	2	0	62,0	68,5	29,72		SW	1	Rain.
25	7	0	56,5	65,0	29,74	0,028	SW	1	Fine.
	2	0	67,0	66,0	29,82		WSW	1	Fair.
26	7	0	58,0	64,0	29,91		NNE	1	Fair.
	2	0	69,5	66,5	29,89		NNE	1	Fair.
27	7	0	57,0	63,5	29,76		NNE	1	Fair.
	2	0	66,0	64,5	29,69		NE	1	Fair.
28	7	0	57,0	61,5	29,62		E by N	1	Fair.
	2	0	59,5	62,5	29,61		SSE	1	Rain.
29	7	0	58,0	62,5	29,70	0,373	SE	1	Fine.
	2	0	63,0	64,0	29,70		SW by W	1	Rain.
30	7	0	59,0	64,5	29,80	0,637	W	1	Fair.
	2	0	67,0	65,5	29,855		SW	1	Fine.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
July 1	7	0	59,5	64,5	29,92	0,263	WNW	1	Fine.
	2	0	71,0	66,5	29,94		W	1	Fine.
2	7	0	60,0	64,5	29,90	0,053	SW	1	Fair.
	2	0	67,5	66,5	29,85		S	1	Rain.
3	7	0	61,0	64,5	29,70	0,386	SW	1	Cloudy.
	2	0	73,0	67,5	29,72		SW by W	1	Fair.
4	7	0	61,0	66,5	29,98	0,024	W	1	Fair.
	2	0	69,5	67,0	30,03		W	1	Fair.
5	7	0	64,0	66,5	30,05		SW	1	Fair.
	2	0	73,0	69,0	30,04		SW by W	1	Fair.
6	7	0	62,5	67,5	29,72	0,091	NE	1	Rain.
	2	0	68,0	70,5	29,59		N	1	Rain, and thunder.
7	7	0	58,0	67,5	29,60	0,359	NNW	1	Rain.
	2	0	64,0	67,5	29,78		NW	1	Fair.
8	7	0	58,0	65,5	29,84	0,058	SW	1	Cloudy.
	2	0	70,5	68,5	29,85		SW	1	Fair.
9	7	0	61,5	65,5	29,70	0,061	S	1	Rain.
	2	0	72,0	68,0	29,74		SW by S	2	Fair.
10	7	0	64,5	67,5	29,84	0,016	WSW	1	Fair.
	2	0	70,5	68,5	29,84		SW by S	2	Fair.
11	7	0	62,0	66,0	29,87	0,010	SW	1	Fine.
	2	0	68,0	68,0	29,85		SSW	2	Fair.
12	7	0	63,5	66,5	29,85	0,020	SW	1	Fair.
	2	0	59,0	66,0	29,86		SSW	1	Rain.
13	7	0	60,5	65,0	29,89	0,677	SW	1	Fair.
	2	0	68,0	67,5	29,89		SW	2	Fair.
14	7	0	62,0	66,0	29,88	0,077	SW	1	Fair.
	2	0	64,0	67,5	29,82		NW	1	Rain.
15	7	0	59,5	65,0	29,81	0,145	NW	1	Fair.
	2	0	66,0	66,5	29,88		NW	1	Fair.
16	7	0	59,0	64,0	29,92	0,024	NW	1	Fair.
	2	0	68,5	66,0	29,92		WNW	1	Fair.

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	Time.		Therm. without	Therm. with n.	Barom.	Rain	Winds.		Weather.
	H.	M.			inches.	Inch.	Points.	Str.	
July 17	7	0	59,0	63,5	30,00		NW	1	Fair.
	2	0	66,5	65,0	30,05		NW	1	Fair.
18	7	0	58,5	61,5	30,16		NNE	1	Fine.
	2	0	73,5	66,5	30,18		NW	1	Fine.
19	7	0	60,0	63,5	30,16		S.W	1	Fine.
	2	0	76,0	68,0	30,14		W by S	1	Fine.
20	7	0	62,0	65,0	30,08		W	1	Fine.
	2	0	79,5	70,5	30,05		SW by W	1	Fine.
21	7	0	69,0	68,5	29,93		SSE	1	Fine.
	2	0	82,0	74,0	29,91		SSE	1	Fine.
22	7	0	66,0	70,0	29,85		NW	0	Hazy.
	2	0	77,0	73,0	29,84		NW	1	Fine.
23	7	0	63,0	70,0	29,82	0,123	NE	1	Cloudy.
	2	0	75,0	73,0	29,80		WSW	1	Fair.
24	7	0	61,0	67,5	29,77	0,310	ENE	0	Fair.
	2	0	74,0	70,5	29,81		SW	1	Rain.
25	7	0	59,5	67,5	29,92	0,069	WSW	1	Cloudy.
	2	0	76,5	70,0	29,91		SW	1	Fair.
26	7	0	67,0	68,5	29,87		S	1	Fair.
	2	0	76,5	70,5	29,84		SW by W	1	Fair.
27	7	0	67,5	69,5	29,78	0,042	SE	1	Fair.
	2	0	75,5	71,0	29,78		SW	1	Fine.
28	7	0	66,0	68,0	29,78		SSE	1	Fair.
	2	0	72,5	69,5	29,72		S	1	Rain.
29	7	0	59,0	66,0	29,70	1,058	SW	1	Thunder at 12 h. fair.
	2	0	66,0	66,5	29,74		S.	1	Fair, much thunder aftern.
30	7	0	60,0	63,5	29,81	0,498	W	1	Fair.
	2	0	69,0	67,0	29,95		WNW	2	Fair.
31	7	0	60,0	63,5	30,04		N	1	Fine.
	2	0	69,5	67,0	30,04		NNW	1	Fine.

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	Time.		Therm.	Therm	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Aug. 1	7	0	61,0	63,5	29,81	0,038	E	1	Rain.
	2	0	72,5	68,0	29,77		WSW	1	Fair.
2	7	0	61,0	64,0	29,73	0,152	SSW	1	Rain.
	2	0	72,5	68,5	29,69		SSW	2	Fair.
3	7	0	62,5	65,0	29,705	0,154	SSW	1	Fine.
	2	0	72,0	68,0	29,73		SSW	1	Fair.
4	7	0	62,0	65,5	29,715	0,218	S	1	Fine.
	2	0	71,0	68,5	29,74		SSW	2	Fine.
5	7	0	61,0	65,5	29,83	0,032	SW	1	Fair.
	2	0	73,5	68,0	29,85		SSW	1	Fine.
6	7	0	60,0	64,5	29,95		NNE	1	Fine.
	2	0	73,0	69,0	29,96		NNW	1	Fine.
7	7	0	62,0	66,5	30,01		W	1	Fair.
	2	0	74,5	69,5	30,01		NW	1	Fine.
8	7	0	61,0	65,5	30,02		W	1	Fine.
	2	0	75,5	70,5	29,99		NW	1	Fine.
9	7	0	64,0	67,0	29,82		SSW	1	Cloudy.
	2	0	66,0	67,5	29,70		SSE	1	Rain.
10	7	0	58,0	63,5	29,88	0,220	WSW	2	Fine.
	2	0	70,5	65,5	29,95		NW	1	Fine.
11	7	30	59,0	63,0	29,88	0,010	S	1	Rain.
	2	0	66,0	66,0	29,80		SSW	2	Cloudy.
12	7	0	57,0	63,0	29,89	0,048	SSW	1	Fine.
	2	0	68,0	66,0	29,82		SSW	1	Cloudy.
13	7	0	57,0	61,5	29,91	0,118	SSW	1	Fair.
	2	0	68,5	64,0	29,92		W	2	Fair.
14	7	0	54,0	60,0	30,04	0,012	WSW	1	Fine.
	2	0	69,5	65,0	30,07		SW	1	Fine.
15	7	0	54,5	60,0	30,05		SE	1	Fog.
	2	0	69,5	64,5	30,00		SE	1	Fine.
16	7	0	56,0	58,0	29,93	0,043	SSE	1	Fair.
	2	30	68,0	63,5	29,98		N	1	Fine.

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	Time.		Therm.	Therm.	Barom.	Rain	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Aug 17	7	0	59,5	62,5	30,02	0,029	S	1	Rain.
	2	0	70,0	65,0	30,02		S by W	2	Cloudy.
18	7	0	66,0	66,0	30,05	0,023	SSW	1	Fair.
	2	0	72,5	69,5	30,06		SSW	1	Fair.
19	7	0	62,0	66,0	29,88		NNW	1	Fair.
	2	0	70,0	68,5	29,80		NW	1	Fair.
20	7	0	57,0	63,5	29,87		SSW	1	Fine.
	2	0	65,5	65,5	29,80		SSE	2	Fair.
21	7	0	52,5	61,0	29,88		SW	0	Fine.
	2	0	65,5	64,0	29,81		S	1	Rain.
22	7	0	59,5	63,0	29,50	0,171	SW by W	2	Fair.
	2	0	69,0	65,5	29,73		W	2	Fine.
23	7	0	56,0	62,5	30,05		SSW	1	Fine.
	2	0	73,0	65,5	30,07		S	1	Fine.
24	7	0	57,0	62,5	30,07		SSW	1	Fine.
	2	0	72,0	66,0	30,04		NW	1	Fine.
25	7	0	55,0	63,0	30,00		NE	1	Fair.
	2	0	74,0	66,0	29,92		E by N	1	Fine.
26	7	0	58,0	64,0	29,74		SE	1	Fair.
	2	0	72,0	67,0	29,66		SSW	1	Fair.
27	7	0	52,5	62,5	29,75	0,358	WSW	1	Cloudy.
	2	0	66,5	65,5	29,76		WSW	2	Cloudy.
28	7	0	60,5	62,5	29,71	0,345	SW	1	Rain.
	2	0	62,5	65,5	29,75		SSW	1	Rain.
29	7	0	56,0	61,0	29,75	0,122	S	1	Fine.
	2	0	67,0	64,5	29,76		S by W	1	Fine.
30	7	0	56,0	61,0	29,77	0,042	SSW	1	Fine.
	2	0	69,0	64,5	29,79		SW	2	Fine.
31	7	0	58,0	62,0	29,54	0,068	SE by E	2	Rain.
	2	0	69,0	66,0	29,52		SSW	2	Fair.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Sept.	1	7 0	56,5	61,0	29,85	0,171	SW	1	Fine.
		2 0	68,0	64,5	29,89		SSW	1	Fine.
	2	7 0	63,0	63,5	29,85	0,115	SWbyW	2	Cloudy, much wind last night
		2 0	67,5	66,5	29,89		SWbyW	2	Cloudy.
	3	7 0	56,5	63,5	30,00	0,020	WSW	1	Fair.
		2 0	56,0	63,5	30,05		S	1	Rain.
	4	7 0	49,5	59,5	30,15	0,170	WSW	1	Fog.
		2 0	65,0	61,5	30,13		SE	1	Fine.
	5	7 0	60,5	61,0	29,88	0,045	ENE	1	Fair.
		2 0	74,5	64,5	29,82		SE	1	Fine.
	6	7 0	64,0	66,0	29,88		WSW	1	Fair.
		2 0	70,5	67,5	29,98		SW	1	Fine.
	7	7 0	63,5	65,5	29,70	0,770	SSW	1	Fair, thunder last night.
		2 0	75,0	68,5	29,71		SW	1	Fine.
	8	7 0	63,0	67,5	29,59	0,026	SSW	1	Fair.
		2 0	65,5	68,5	29,54		SW	1	Rain.
	9	7 0	59,0	65,0	29,54	0,435	SSW	1	Fine.
		2 0	67,0	67,0	29,61		SSW	2	Cloudy.
	10	7 0	57,5	63,5	29,61	0,283	SSE	1	Rain.
		2 0	68,5	66,5	29,61		SWbyW	1	Fair.
	11	7 0	53,5	61,0	29,64	1,100	WSW	1	Fine.
		2 0	60,5	62,5	29,64		SSW	2	Rain.
	12	7 0	47,0	56,0	29,90	0,097	WSW	1	Fine.
		2 0	60,5	59,0	29,91		W by S	1	Fine.
	13	7 0	53,5	67,5	29,57	0,378	W	1	Fair.
		2 0	59,5	59,5	29,57		NW	1	Rain.
	14	7 0	52,0	57,0	29,70	0,512	N	1	Rain.
		2 0	62,0	60,0	29,78		N by E	1	Fair.
	15	7 0	53,5	56,5	29,68	0,184	NW	1	Fair.
		2 0	61,5	60,5	29,63		NNE	1	Cloudy.
	16	7 0	54,0	57,5	29,59	0,497	NNE	1	Fair.
		2 0	62,0	59,5	29,71		WNW	1	Fine.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Sept. 17	7	0	50,5	56,5	29,84		SW	1	Fair.
	2	0	65,0	60,0	29,83		SSW	1	Fine.
18	7	0	58,0	58,0	29,63		SSW	1	Cloudy.
	2	0	63,0	60,5	29,67		S by W	1	Cloudy.
19	7	0	62,0	61,5	29,70	0,282	S	1	Rain.
	2	0	66,0	63,5	29,79		SW by W	1	Cloudy.
20	7	0	61,0	62,5	29,81	0,018	S	1	Fair.
	2	0	71,0	66,0	29,74		S	1	Fine.
21	7	0	61,0	63,5	29,51		S	1	Cloudy.
	2	0	66,5	65,0	29,60		S	2	Fine.
22	7	0	56,0	61,0	29,88	0,089	S	1	Fine.
	2	0	70,5	65,5	29,95		ESE	1	Fine.
23	7	0	56,5	60,5	29,90		ENE	1	Fine.
	2	0	69,5	66,0	29,87		ESE	1	Fine.
24	7	0	58,0	61,5	29,78		NE	1	Fine.
	2	0	71,5	67,0	29,75		ENE	1	Fine.
25	7	0	58,5	62,0	29,73		NE	1	Fair.
	2	0	70,5	66,5	29,72		E	1	Fine.
26	7	0	53,5	60,5	29,69		NNE	1	Fine.
	2	0	70,0	66,0	29,69		SSW	1	Fine.
27	7	0	57,5	62,5	29,79		WSW	1	Fair.
	2	0	64,0	64,0	29,85		W	1	Fair.
28	7	0	47,0	59,0	29,84		SW	1	Fine.
	2	0	63,0	61,5	29,84		SSW	1	Fair.
29	7	0	50,5	59,5	29,84		WSW	1	Fair.
	2	0	63,5	62,0	29,855		W	1	Fair.
30	7	0	50,0	58,5	29,82		SW	0	Fair.
	2	20	63,0	61,0	29,85		N by E	1	Fine.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
O&.	1	7 0	50,0	57,5	29,92		NE	1	Fair.
		2 0	64,5	61,0	29,94		NNE	1	Fine.
2	7 0	49,5	54,5	29,80			NE	0	Fog.
	2 0	63,5	59,0	29,68			NE by E	1	Fair.
3	7 0	56,0	58,5	29,40	0,037		N	1	Fair.
	2 0	63,0	60,0	29,42			WSW	1	Fine.
4	7 0	57,5	57,5	29,555			W	1	Cloudy.
	2 0	58,0	58,5	29,68			W	1	Fine.
5	7 0	40,5	52,5	29,82			SSW	1	Fine.
	2 0	57,0	54,0	29,81			W by N	1	Fine.
6	7 0	49,0	51,5	29,73			SSW	1	Cloudy.
	2 0	55,5	54,0	29,70			WSW	1	Rain.
7	7 0	51,0	52,5	29,63	0,257		S by W	1	Rain.
	2 0	53,0	54,0	29,76			W	1	Fine.
8	7 0	42,5	48,5	30,07	0,178		SW	1	Fine.
	2 0	57,5	52,5	30,09			WSW	1	Fine.
9	7 0	59,5	54,5	29,77	0,191		SW	1	Rain.
	2 0	66,0	57,5	29,84			SW	1	Fair.
10	7 0	47,5	55,5	30,00			SW	1	Fine.
	2 0	61,0	58,5	30,05			SW	1	Fair.
11	7 0	50,5	56,0	30,06			SW	1	Fair.
	2 0	62,5	68,0	30,07			WSW	1	Fine.
12	7 0	58,5	58,5	29,95			SSW	1	Cloudy.
	2 0	63,5	60,0	29,92			SW	1	Fair.
13	7 0	56,0	60,5	29,82	0,027		SW	1	Fair.
	2 0	63,0	61,5	29,84			WSW	1	Fair.
14	7 0	46,5	55,0	29,91			SSW	1	Fine.
	2 0	58,0	57,5	29,91			SW	1	Fine.
15	7 0	43,5	52,5	29,98			SW	1	Fair.
	2 0	57,0	54,5	30,05			W	1	Fair.
16	7 0	41,0	51,0	30,21			S	1	Fog.
	2 0	57,0	53,5	30,13			SW by W	1	Fine.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Oct 17	7	0	55,0	54,0	29,95	0,112	W	0	Fog.
	2	0	62,0	57,0	29,89		SW	0	Fair.
18	7	0	51,0	56,5	29,43	0,612	SW	1	Fair.
	2	0	55,0	57,5	29,48		W W	2	Fair.
19	7	0	41,5	53,0	29,70	0,208	SSW	1	Fine.
	2	0	53,0	53,5	29,42		S	2	Cloudy.
20	7	0	43,0	50,0	29,16	0,369	S by W	3	Windy night.
	2	0	49,0	50,5	29,23		WSW	2	Fine.
21	7	0	43,0	48,5	29,20		SW	1	Fine.
	2	0	54,5	50,5	29,55		WSW	2	Fine.
22	7	0	43,0	48,5	29,77	0,019	SW	1	Cloudy.
	2	0	44,5	49,0	29,69		ENE	1	R.in.
23	7	0	39,0	46,5	30,09	0,348	NW	1	Fine.
	2	0	51,0	48,5	30,20		NW	1	Fair.
24	7	0	42,5	47,5	30,05		S by W	1	Fair.
	2	0	47,0	49,5	29,98		NW	2	Rain.
25	7	0	37,0	45,0	30,04	0,010	N	1	Fine.
	2	0	44,0	45,5	30,16		NW	1	Fine.
26	7	0	32,0	41,5	30,25		W	1	Fine.
	2	0	46,0	43,0	30,26		NNW	1	Fine.
27	7	0	39,0	43,0	30,24		W	1	Fair.
	2	0	48,0	45,0	30,19		WSW	1	Fine.
28	7	0	44,0	45,0	29,98		SW	1	Cloudy.
	2	0	52,0	47,5	29,91		W	1	Fair.
29	7	0	40,0	46,5	30,00	0,020	N	1	Fine.
	3	30	48,0	47,0	30,07		NNW	1	Fair.
30	7	0	34,0	44,0	30,13		SSW	1	Fair.
	2	0	45,0	44,5	30,14		S	1	Fair.
31	7	0	41,0	44,0	29,94	0,039	E	0	Rain.
	2	0	47,0	45,5	29,80		SSW	1	Rain.

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	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Nov. 1	8	0	51,0	48,0	29,59	0,985	SSW	0	Rain.
	2	0	55,5	50,5	29,63		WNW	1	Fair.
2	8	0	41,0	46,0	29,75		NE	1	Fair.
	2	0	46,0	48,5	29,81		ENE	2	Fine.
3	8	0	40,0	44,5	29,84		ENE	1	Fair.
	2	0	46,5	46,0	29,84		ENE	1	Fair.
4	8	0	41,5	43,5	29,77		NE	1	Fair.
	2	0	46,0	45,0	29,72		ENE	1	Fair.
5	8	0	42,0	43,5	29,57	0,057	ENE	1	Rain.
	2	0	42,5	45,0	29,50		ENE	1	Rain.
6	8	0	46,0	46,0	29,42	0,133	ENE	1	Rain.
	2	0	48,0	47,5	29,37		ESE	1	Rain.
7	8	0	45,5	48,5	29,38	0,194	NE	1	Rain.
	2	0	47,0	49,0	29,33		NE	1	Rain.
8	8	0	46,0	48,5	29,41		N	1	Cloudy.
	2	0	48,5	49,5	29,49		NNW	1	Rain.
9	8	0	42,0	47,0	29,70		N	1	Fair.
	2	0	46,0	48,0	29,86		N by E	1	Fine.
10	8	0	33,0	44,5	30,00		SW	1	Fair.
	2	0	42,0	45,0	30,00		SW	1	Fair.
11	8	0	38,0	42,5	29,78		E	1	Fog.
	2	0	47,5	44,5	29,68		E	1	Fair.
12	8	0	45,5	45,5	29,49	0,063	ESE	1	Cloudy.
	2	0	57,5	47,5	29,37		SW	1	Rain.
13	8	0	53,5	51,5	29,01	0,119	S by W	2	Rain.
	2	0	55,0	53,0	29,00		WSW	1	Cloudy.
14	8	0	40,0	46,5	29,41	0,211	NW	2	Fine.
	2	0	44,5	46,5	29,57		NW by W	2	Fine.
15	8	0	33,5	42,0	29,69		SW	1	Fair.
	2	0	37,5	42,5	29,62		NW	1	Rain.
16	8	0	31,0	40,0	29,64	0,163	N	1	Fine.
	2	0	40,0	40,5	29,78		N	1	Fine.

METE-

for 1775.

	Time.		Therm.	Therm.	barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Nov. 17	8	0	37,5	40,0	29,68	0,019	SE	1	Rain.
	2	0	46,5	41,0	29,27		SSW	1	Rain.
18	8	0	41,0	44,0	29,16	0,758	NW	2	Cloudy.
	2	0	41,5	44,0	29,41		N	2	Cloudy.
19	8	0	35,0	40,5	30,05	0,048	N	1	Fine.
	2	0	39,0	41,0	30,14		N	1	Fine.
20	8	0	35,0	39,0	30,15		S by W	1	Fair.
	2	0	41,0	40,5	30,13		SSE	1	Cloudy.
21	8	0	40,0	41,0	30,30	0,072	SE	1	Cloudy.
	2	0	42,5	40,0	30,33		E	1	Fair.
22	8	0	28,0	37,0	30,40		N	1	Fine.
	2	0	36,5	38,0	30,36		NNE	1	Fair.
23	8	0	36,5	38,0	30,17	0,040	SW	1	Fog.
	2	0	40,5	39,0	30,15		NE	1	Cloudy.
24	8	0	36,0	38,0	30,15	0,025	NNE	1	Fair.
	2	0	41,0	39,5	30,12		NE	1	Fair.
25	8	0	36,0	40,0	30,06		NE	1	Fair.
	2	0	40,0	40,5	30,02		NNE	1	Cloudy.
26	8	0	35,5	40,0	30,01	0,025	SE	1	Cloudy.
	2	0	38,0	40,0	30,01		SSE	1	Rain.
27	8	0	37,5	40,0	29,99		E	1	Fog.
	2	0	39,5	40,5	29,94		ESE	1	Fair.
28	8	0	37,5	39,5	29,80		ENE	1	Rain.
	2	0	40,5	41,0	29,77		SE	1	Rain.
29	8	0	45,0	42,5	29,67	0,010	SSE	1	Fair.
	2	0	49,5	44,0	29,55		S	1	Rain.
30	8	0	54,5	48,0	29,89	0,019	SE	1	Fair.
	2	0	57,0	50,0	29,90		SSW	1	Fair.

METE-

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for 1775.

	Time.	Therm. without	Therm within	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
Dec. 1	8 0	55 0	54,5	30,18		S	1	Fair.
	2 0	58,0	55,0	30,19		S	1	Fine.
2	8 0	51,0	54,0	30,20		SW	1	Fair.
	2 0	53,5	56,0	30,18		SW	1	Fine.
3	8 0	50,0	53,0	30,21		SW	1	Fair.
	2 0	55,5	54,5	30,22		WSW	1	Fair.
4	8 0	52,0	54,5	30,33		WSW	1	Fog.
	2 0	53,0	54,0	30,36		WSW	1	Cloudy.
5	8 0	47,5	53,5	30,41		WSW	1	Cloudy.
	2 0	49,5	53,0	30,40		SW	1	Cloudy.
6	8 0	42,0	48,0	30,46		N	1	Fine.
	2 0	47,0	50,0	30,48		N	1	Fine.
7	8 0	38,0	46,5	30,57		SSW	1	Fog.
	2 0	44,0	46,5	30,53		N by W	1	Fair.
8	8 0	45,0	46,5	30,45		WSW	1	Fog.
	2 0	49,0	47,5	30,40		SW	1	Cloudy.
9	8 0	37,0	44,0	30,45		N	1	Fine.
	2 0	41,5	44,0	30,49		N	1	Fine.
10	8 0	33,0	40,0	30,55		NNE	1	Fine.
	2 0	38,5	40,5	30,57		NNE	1	Fine.
11	8 0	32,5	37,5	30,51		SW	1	Fog.
	2 0	41,0	39,0	30,48		SSW	1	Fine.
12	8 0	34,5	38,0	30,45		SSW	1	Fine.
	2 0	40,0	40,0	30,42		S	1	Fine.
13	8 0	27,0	35,5	30,33		SW	1	Fine.
	2 0	40,0	37,0	30,26		SW	1	Fair.
14	8 0	46,5	40,0	30,20	0,030	W	0	Fog.
	2 0	48,0	43,0	30,24		N	1	Fair.
15	8 0	33,5	40,5	30,33		ENE	1	Fair.
	2 0	36,0	41,0	30,32		NNE	1	Fair.
16	8 0	34,5	38,0	30,40		NE	0	Foggy.
	2 0	41,0	39,0	30,41		NE	1	Foggy.

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for 1775.

	Time.		Therm.	Therm.	Barom.	Rain	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Dec. 17	8	0	30,5	36,5	30,45		E	1	Fine.
	2	0	34,0	37,0	30,55		SE	1	Fine.
18	8	0	31,0	34,0	30,535		ENE	1	Fair.
	2	0	35,5	34,5	30,47		NE	1	Fine.
19	8	0	29,0	34,0	30,32		NE	1	Fair.
	2	0	37,5	35,0	30,29		ENE	1	Fog.
20	8	0	35,0	34,0	30,12		E	1	Fog.
	2	0	36,0	37,0	29,98		SE	1	Fair.
21	8	0	37,5	37,0	29,38		SSE	1	Fog.
	2	0	46,5	39,0	29,21		SSW	1	Fair.
22	8	0	41,0	42,5	29,30	0,050	SW	1	Fog.
	2	0	46,0	43,5	29,41		SW	1	Fine.
23	8	0	48,5	45,5	29,13	0,146	SSW	2	Rain.
	2	0	51,0	47,0	29,06		SSW	2	Fine.
24	8	0	45,0	46,0	28,80	0,114	SE by E	2	Rain.
	2	0	47,0	47,0	28,60		SSW	2	Rain.
25	8	0	38,5	44,0	28,97	0,035	SSE	1	Fine.
	3	0	45,5	47,0	28,99		SW	1	Fair.
26	8	0	40,0	43,0	29,42		WSW	0	Fair.
	2	0	43,5	43,5	29,50		SW	1	Fine.
27	8	0	33,0	41,5	29,80		SSW	1	Fog.
	2	0	40,0	42,0	29,86			1	Fair.
28	8	30	29,0	38,5	29,96		NNE	1	Foggy.
	2	0	38,0	39,0	29,86		SSE	1	Fair.
29	8	0	39,0	39,5	29,68	0,144	SSW	1	Rainy.
	2	0	43,5	41,5	29,81		NNW	1	Fair.
30	8	0	30,5	38,0	29,96		NNE	1	Foggy.
	2	0	40,0	39,0	29,91		S	1	Fine.
31	8	0	32,0	38,0	30,12	0,057	SSW	1	Fair.
	2	0	38,5	38,5	30,14		S	1	Fair.

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	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
						Points.	Str.	
	H. M.			Inches.	Inch.			
Jan. 1	8 0	38,5	39,0	29,94		SE	1	Cloudy.
	2 0	41,0	40,0	29,82		S	1	Fine.
2	8 0	41,0	41,5	29,50	0,075	NNW	1	Rain.
	2 0	40,5	41,5	29,69		NNW	1	Cloudy.
3	8 0	36,0	39,5	30,09	0,065	NE	1	Cloudy.
	2 0	37,0	40,5	30,14		E	1	Fine.
4	8 0	39,0	38,5	29,77	0,031	SSE	1	Rain.
	2 0	44,0	40,0	29,50		S	2	Rain.
5	8 0	44,5	43,5	29,40	0,444	SW	1	Fair.
	2 0	48,5	45,5	29,48		WSW	1	Cloudy.
6	8 0	36,0	43,5	29,63		WSW	1	Fine.
	2 10	41,5	44,0	29,57		E by N	1	Fair.
7	8 30	33,0	39,5	29,245	0,153	ENE	2	Rain.
	2 0	31,0	38,0	29,35		ENE	2	Snow.
8	8 0	30,0	34,0	29,46	0,229	NE	1	Much snow left night.
	2 0	32,0	35,5	29,49		N	1	Fair.
9	8 0	30,0	34,5	29,63	0,079	N	1	Cloudy.
	2 0	28,5	33,5	29,67		ENE	1	Cloudy.
10	8 0	30,5	33,5	29,73		NNE	1	Cloudy.
	2 0	33,0	34,0	29,73		NW	1	Cloudy.
11	8 0	30,0	31,5	29,57		E	1	Cloudy.
	2 0	35,0	33,5	29,41		SE	1	Cloudy.
12	8 0	30,0	33,0	29,21	0,091	E	2	Much snow and wind left nt.
	2 0	32,0	34,0	29,21		ENE	2	Snow.
13	8 0	30,0	32,5	29,32		NE	2	Much snow and wind left nt.
	2 0	29,0	32,0	29,36		NE	2	Cloudy.
14	8 0	25,0	30,0	29,45		ENE	2	Snow.
	2 0	26,0	29,0	29,49		NE	2	Snow.
15	8 0	26,0	29,0	29,70		N	1	Cloudy.
	2 0	29,0	30,5	29,70		ENE	1	Snow.
16	8 0	25,5	29,0	29,71		NNE	1	Snow.
	2 0	30,5	30,5	29,70		NE	1	Snow.

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	Time.		Therm.	Therm.	Baion.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inchs.	Inch.	Points.	Str.	
Jan 17	8	0	28,0	29,5	29,74		NNE	1	Cloudy.
	2	0	30,5	30,5	29,74		NNE	1	Cloudy.
18	8	0	33,0	32,0	29,87		ENE	1	Cloudy.
	2	0	30,0	31,5	29,90		ENE	1	Cloudy.
19	8	0	27,0	30,5	29,90		NE	1	Snow.
	2	0	27,0	31,0	29,85		NE	1	Fair.
20	8	0	23,5	29,0	29,70		N	1	Fair.
	2	0	24,0	29,0	29,70		N	1	Fine.
21	8	0	22,0	27,0	29,62		SSW	1	Cloudy.
	2	0	28,5	27,5	29,55		SSW	1	Fine.
22	8	0	30,5	28,5	29,41		E	1	Fair.
	2	0	33,5	30,5	29,39		NE	1	Fine.
23	8	0	25,0	28,5	29,53		NE	1	Fair.
	2	0	33,0	30,0	29,58		NNE	1	Snow.
24	8	0	30,0	30,0	29,81		ENE	0	Cloudy.
	2	0	35,0	31,5	29,84		NE by E	1	Fair.
25	8	0	29,0	31,5	29,87		NE	1	Cloudy.
	2	0	33,0	32,5	29,85		SE	1	Fair.
26	8	0	26,0	30,5	29,82		E	1	Cloudy.
	2	0	26,0	30,5	29,82		SSE	2	Fine.
27	8	0	19,5	26,0	29,80		ENE	2	Snow.
	2	0	20,5	25,5	29,79		ENE	2	Snow.
28	8	0	18,5	22,5	29,94		E	2	Snow.
	2	0	22,0	23,0	30,00		E	2	Fine.
29	8	0	14,5	20,0	29,91		SE	1	Fine.
	2	0	24,0	21,5	29,88		SE by E	1	Fair.
30	8	0	15,0	19,5	29,97		NE	1	Fair.
	2	0	21,0	20,5	30,02		E	1	Fine.
31	8	0	13,5	19,5	30,09		NE	1	Fair.
	2	0	23,5	20,5	30,07		E	1	Fine.

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for February 1776.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Feb. 1	8	0	14,5	19,0	29,97		NE	1	Fair.
	2	0	31,5	31,5	29,86		E	1	Cloudy.
2	8	0	37,0	28,5	29,78		NE	0	Cloudy.
	2	0	40,5	31,0	29,57		SE by E	1	Fair.
3	8	0	37,0	34,5	29,34	0,093	SSE	1	Fog.
	2	0	44,0	36,0	29,51		S by W	1	Fine.
4	8	20	44,0	38,5	29,22		SW	2	Cloudy.
	2	0	48,0	41,0	29,22		SW	1	Cloudy.
5	8	0	40,5	42,0	29,16	0,153	WSW	2	Fine.
	2	0	48,0	44,0	29,11		SW by W	2	Fine.
6	8	0	37,0	42,5	28,98	0,221	WSW	2	Fine.
	2	0	45,0	44,0	29,02		WSW	2	Fine.
7	8	0	43,0	42,0	28,91		SW by W	2	Fair.
	2	0	47,5	43,5	29,07		SW	2	Fair.
8	8	0	40,0	42,5	29,66	0,190	SW	1	Rain.
	2	0	49,5	44,5	29,62		SSW	2	Cloudy.
9	8	0	41,0	44,5	29,78	0,190	SSW	1	Fine.
	2	0	45,5	45,5	29,58		S	2	Cloudy.
10	8	0	42,0	46,0	28,99	0,524	SW by W	2	Rain.
	2	0	48,0	47,0	28,97		SW	2	Fair.
11	8	0	42,5	45,0	28,84	0,275	SW	2	Fair.
	2	0	45,5	46,0	28,84		SSW	2	Rain.
12	8	0	34,0	42,0	29,10		SSW	1	Fair.
	2	0	46,0	44,0	29,24		WSW	1	Fine.
13	8	0	36,5	42,5	29,68		SW	1	Fine.
	2	0	47,0	44,0	29,71		WSW	1	Fine.
14	8	0	47,0	45,0	29,51	0,163	SW	2	Fair.
	2	0	51,0	48,0	29,53		S by W	1	Fine.
15	8	0	38,0	44,5	29,96		SSW	1	Fine.
	2	0	48,0	46,5	29,91		SSW	2	Fair.
16	8	0	42,0	46,0	29,75	0,085	SW	1	Fine.
	2	0	49,5	47,5	29,78		SW	1	Fair.

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	Time.		Therm.	Therm.	Barom	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points	Str.	
Feb. 17	8	0	36,0	43,0	29,68	0,021	ENE	1	Rainy.
	2	0	43,5	42,5	29,53		E	1	Fair.
18	7	0	35,5	42,0	29,43	0,270	SSW	1	Fair.
	2	0	39,5	42,0	29,25		NE	1	Cloudy.
19	7	0	38,5	42,0	29,34	0,140	SW	1	Rainy.
	2	0	41,5	41,5	29,62		NW	1	Fair.
20	7	0	41,5	40,5	29,775	0,089	S	1	Rainy.
	2	0	50,0	44,0	29,80		W by S	1	Fine.
21	7	0	49,5	45,5	29,58		WSW	2	Cloudy.
	2	0	54,5	49,0	29,50		WSW	2	Fair.
22	7	0	36,5	44,0	29,62	0,412	SW	1	Fine.
	2	0	45,0	45,0	29,41		SSE	3	Rain.
23	7	0	35,0	40,5	29,38	0,148	WSW	1	Fine.
	2	0	45,5	44,0	29,50		WSW	2	Fair.
24	7	0	39,0	41,0	29,25	0,079	SSE	1	Rain.
	2	0	46,0	48,0	29,10		WSW	1	Rain.
25	7	0	37,0	40,0	29,50	0,102	N	1	Cloudy.
	2	0	43,0	41,5	29,55		SSW	1	Fair.
26	7	0	46,0	43,5	29,18		SSW	1	Cloudy.
	2	0	53,0	46,5	29,13		S by W	2	Cloudy.
27	7	0	39,5	43,0	29,155	0,149	SSW	2	Cloudy.
	2	0	48,0	45,0	29,26		WSW	2	Fair.
28	7	0	43,0	43,5	29,11	0,140	SSW	2	Fair.
	2	0	47,0	45,5	29,17		SW	2	Fair.
29	7	0	38,0	43,0	29,35	0,066	SSW	1	Fine.
	2	0	48,0	45,0	29,35		WSW	1	Fine.

1775.	Thermometer without.			Thermometer within.			Barometer.		
	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.
January,	54,0	25,5	42,7	52,0	30,5	43,5	30,38	29,30	29,84
February,	55,0	35,0	44,5	52,5	41,5	45,7	30,48	28,89	29,24
March,	58,0	28,5	43,9	53,0	37,0	46,1	30,61	29,07	29,67
April,	83,5	36,5	52,8	69,0	40,0	52,7	30,36	29,50	30,026
May,	74,5	43,0	57,8	67,5	50,0	58,5	30,43	29,68	30,12
June,	81,5	52,0	66,3	73,5	60,5	67,3	30,30	29,61	29,91
July,	82,0	58,0	66,1	74,0	61,5	67,3	30,18	29,59	29,88
August,	75,5	52,5	64,3	70,5	58,0	64,8	30,07	29,50	29,86
September,	75,0	47,0	61,1	68,5	56,0	62,6	30,15	29,51	29,756
October,	66,0	32,0	50,5	68,0	41,5	52,4	30,26	29,16	29,86
November,	57,0	28,0	42,5	53,0	37,0	43,7	30,36	29,16	29,76
December,	58,0	27,0	41,2	56,0	34,0	43,0	30,57	28,60	30,06
1776.									
January,	44,5	13,5	29,3	43,5	20,5	31,8	30,14	29,21	29,687
February,	49,5	14,5	42,6	46,0	19,0	42,4	29,97	28,84	29,408
Whole year, beginning with March 1775.			51,5			52,7			29,833

VARIATION-NEEDLE.

	7 h. A.M.	12 h. M.	2 h. P.M.	10 or 11 h. P.M.	Daily Mean.
June 18	21 34	21 48	21 45	21 36	21 41
19	21 31	21 53	21 55	21 38	21 46
20	21 35	21 53	21 50	21 41	21 45
21	21 39		21 50	21 41	21 43
22	21 39	21 50	21 50	21 40	21 45
23	21 37	21 47	21 50	21 41	21 44
24	21 44	21 45	21 52	21 47	21 47
25	21 40	21 45	21 46	21 40	21 43
26	21 40	21 47	21 47	21 30	21 41
27	21 30	21 46	21 46	21 43	21 41
28	21 37	21 47	21 47	21 40	21 43
29	21 38	21 39	21 45	21 38	21 40
30	21 37	21 43	21 47	21 40	21 44
July 1	21 34	21 44	21 47	21 38	21 41
2	21 37	21 47	21 48	21 36	21 42
3	21 42	21 45	21 48	21 40	21 44
4	21 37	21 47	21 45	21 42	21 43
Means,	21 37	21 47	21 48	21 39	
Mean of all 21 43.					

DIPPING-NEEDLE.

		7 h. A.M.	12 h. M.	2 h. P.M.	11 h.	Mean.
Divided face of instrument to the West,	June 19	72° 30'	72° 33'	72° 28'	72° 32'	72° 32'
	20	72° 30'	72° 28'	72° 32'	72° 30'	
	21	72° 38'		72° 32'	72° 25'	
	22	72° 27'	72° 38'	72° 32'	72° 30'	
	23	72° 26'	72° 30'	72° 28'	72° 32'	
	24	72° 38'	72° 28'	72° 30'	72° 26'	
	25	72° 34'	72° 37'	72° 40'	72° 32'	
East,	26	72° 33'	72° 33'	72° 34'	72° 24'	72° 32'
	27	72° 28'	72° 33'	72° 32'	72° 33'	
	28	72° 35'	72° 35'	72° 34'	72° 32'	
	29	72° 32'	72° 30'	72° 44'	72° 33'	
June 29, Poles changed, marked end points upwards.						
East,	June 29			72° 44'	72° 33'	72° 40'
	30	72° 35'	72° 45'	72° 45'	72° 45'	
West,	July 1	72° 33'	72° 45'	72° 45'	72° 32'	72° 17'
	2	72° 24'	72° 15'	72° 17'	72° 20'	
	3	72° 14'	72° 13'	72° 12'	72° 15'	
	4	72° 17'	72° 18'	72° 18'	72° 18'	
Mean of the four means, or the true dip, 72° 30'.						

PHILOSOPHICAL
TRANSACTIONS.

PART II.

VOL. LXVI.

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PHILOSOPHICAL
TRANSACTIONS,
OF
THE ROYAL SOCIETY
OF
LONDON.

VOL. LXVI. For the Year 1776.

PART II.

LONDON,

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PRINTER TO THE ROYAL SOCIETY.

MDCCLXXVII.

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PHILOSOPHICAL
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PART II.

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XVIII. *An abridged State of the Weather at London for One Year, commencing with the Month of March 1775, collected from the Meteorological Journal of the Royal Society. By S. Horsley, LL. D. Sec. R. S.*

R. June 27, 1776.

T A B L E I.

An abridged view of the WINDS at LONDON,
for one year, beginning with March 1775.

	N	S	E	W	NW	SE	NE	SW		Rain.		Quarterly Rain.	Half-yearly Rain.
Mar.	1	1½	0	3	6½	1	1	17	31	1,854			
Apr.	1½	3½	0½	1½	3½	3	6½	10	30	1,068		3,474	
May	6	0	0	2½	6½	2	6½	7½	31	0,552			11,298
June	1	1	0½	1½	1	4½	14	6½	30	1,389			
July	1	2½	0	2	7½	2	2	14	31	4,232		7,824	
Aug.	0½	3	0½	2	3½	3½	1½	16½	31	2,203			
Sept.	0½	3½	0½	1½	1½	2½	5	15	30	5,192			
Oct.	1½	1½	0½	4	3½	0	2½	17½	31	2,919		10,560	
Nov.	4	0½	2	0	3	5½	9½	5½	30	2,449			
Dec.	2½	2½	1	0½	1	3	6	14	30½	0,576	{ Half day missed in the Journ.		
Jan.	2½	1	4½	0	1½	3½	15½	2½	31	1,167			15,813
Feb.	0½	1	1	0	0½	2	2	22	29	3,510		5,253	
	22½	21½	11	18½	39½	32½	72	148		27,111			

It appears, that the winds from the S.W. were again the most frequent of any, and next to these the winds from the N.E. Of the winds from the four cardinal points, the North was the most frequent, and the East the most rare. The autumn was the wettest quarter, and the spring the driest. The rain of the three summer months was almost half as much again as that of the three winter months; but the rain of the winter half-year exceeded that of the summer half-year by about one-sixth of the rain of the whole year. September gave the greatest quantity of rain, and May the least of any single month in the whole year.

TABLE II.

Sub-division of the S.W.

	WSW	SW	SSW	
March	5	5	7	17
April	3	4	3	10
May	$3\frac{1}{2}$	$0\frac{1}{2}$	$3\frac{1}{2}$	$7\frac{1}{2}$
June	3	3	$0\frac{1}{2}$	$6\frac{1}{2}$
July	4	7	3	14
August	3	$2\frac{1}{2}$	11	$16\frac{1}{2}$
September	6	$3\frac{1}{2}$	$5\frac{1}{2}$	15
October	5	$7\frac{1}{2}$	5	$17\frac{1}{2}$
November	$0\frac{1}{2}$	$2\frac{1}{2}$	$2\frac{1}{2}$	$5\frac{1}{2}$
December	3	6	5	14
January	1	$0\frac{1}{2}$	1	$2\frac{1}{2}$
February	8	$6\frac{1}{2}$	$7\frac{1}{2}$	22
	45	$48\frac{1}{2}$	$54\frac{1}{2}$	148

TABLE III.

Sub-division of the N.E.

	ENE	NE	NNE	
March	0	1	0	1
April	1	3	$2\frac{1}{2}$	$6\frac{1}{2}$
May	2	$1\frac{1}{2}$	3	$6\frac{1}{2}$
June	6	4	4	14
July	$0\frac{1}{2}$	1	$0\frac{1}{2}$	2
August	$0\frac{1}{2}$	$0\frac{1}{2}$	$0\frac{1}{2}$	$1\frac{1}{2}$
September	$1\frac{1}{2}$	1	$2\frac{1}{2}$	5
October	1	1	$0\frac{1}{2}$	$2\frac{1}{2}$
November	4	$3\frac{1}{2}$	2	$9\frac{1}{2}$
December	$1\frac{1}{2}$	2	$2\frac{1}{2}$	6
January	$6\frac{1}{2}$	$6\frac{1}{2}$	$2\frac{1}{2}$	$15\frac{1}{2}$
February	$0\frac{1}{2}$	$1\frac{1}{2}$	0	2
	25	$26\frac{1}{2}$	$20\frac{1}{2}$	72

T A B L E I V.

Sub-division of the S.E.

	ESE	SE	SSE	
March	0	0½	0½	1
April	1	1	1	3
May	0	1½	0½	2
June	1½	1½	1½	4½
July	0	0½	1½	2
August	0½	1½	1½	3½
September	1	1	0½	2½
October	0	0	0	0
November	1½	2½	1½	5½
December	0½	1	1½	3
January	0½	2	1	3½
February	0½	0	1½	2
	7	13	12½	32½

T A B L E V.

Sub-division of the N.W.

	WNW	NW	NNW	
March	0½	3½	2½	6½
April	1½	1	1	3½
May	1½	4	1	6½
June	0	1	0	1
July	1½	5	1	7½
August	0	2½	1	3½
September	0½	1	0	1½
October	0½	2	1	3½
November	1	1½	0½	3
December	0	0	1	1
January	0	0½	1	1½
February	0	0½	0	0½
	7	22½	10	39½

Of the winds between the S. and W. those from the S.S.W. were this year the most frequent.

Here follows a general state of the winds, according to the degrees in which they prevailed respectively, collected from the five preceding tables.

ESE	WNW	NNW	E	SSE	SE	W	NNE	S	NW	N	ENE	NE	WSW	SW	SSW	
7	7	10	11	12½	13	18½	20½	21½	22½	22½	25	26½	45	48½	54½	365½

Missed in the journal,

TABLE VI.

Shewing the number of fair and frosty days in each half-month and in the whole year.

	Fair		Fair days in whole month.	Frosty days		Frosty days in whole months.
	1st half.	Latter half.		1st half.	Latter half.	
March	6	7	13			
April	15	9	24			
May	13	13	26			
June	12	11	23			
July		9	10			
	5	7	12			
September	1	11	12			
October	11	6	17		1	
November	8	5	13		1	2
	15	9	24		6	8
January	5	11	16	10	15	25
February	7		9			
Total fair days,			199	Total frosty days,		41

There were eleven snowy days in this year, all in January, with the wind between the N. and E. The first snow fell on the 7th, and introduced the great frost, which set in in the day-time: for on the 7th, at 8½ in the morning, it rained with the ther-

mmometer at 33°, wind E.N.E.; but, at 2 in the afternoon of the same day, the rain was turned into snow, and the thermometer was sunk to 31°. There was a short remission of the frost on the 18th, the thermometer at 8 in the morning of that day being at 33°; but it was sunk again to 30° at 2 in the afternoon. On the 31st, at 8 in the morning, it was at 13.5, and only one degree higher the next morning, February 1st. The frost broke in the night between

between the 1st and 2d of February, the wind yet continuing N.E., from which quarter it had set almost all the time the frost lasted. It changed to the S.E. on the 2d, and on the 3d got into the S.W. where it remained almost all the rest of the month.

The following table shews the quantity of rain that fell with each wind in each month and in the whole year. It appears, that the S.W. gave more than two-thirds of the rain of the whole year, which seems not to have been altogether owing to the wet quality of that wind, but in great measure to the greater length of time it blew than any other. The numbers at the bottom of the table shew the proportional wetness of each wind upon the whole. They are made from the numbers in the last horizontal row but one of this table compared with the numbers in the last horizontal row of TAB. I. For the wetness of each wind is in proportion as the quantity of rain it gave in the whole year directly, and the number of days it blew inversely. The former is shewn by the numbers in the last row but one of TAB. VII.; and the latter by those in the last row of TAB. I. It appears, that the South wind was the driest of all, the S.W. the wettest, and the W. the next wettest.

TABLE VII.

Shewing the quantities of rain which fell severally with each wind in every month and in the whole year.

	N	S	E	W	NW	SE	NE	SW	
Mar.	0	0		0,132	0,114	0,074	0,095	1,439	1,854
Apr.	0	0,016	0	0,386	0,143	0	0	0,523	1,068
May	0			0,058	0	0	0,052	0,442	0,552
June	0	0		0,768	0	0,495	0,098	0,028	1,389
July	0	0		0,155	0,528	0	0,214	3,335	4,232
Aug.	0	0,039	0,038	0	0	0,043	0	2,083	2,203
Sept.	0	0,107	0	0,378	0,184	0	0,542	3,981	5,192
Oct.	0,037	0	0,039	0	0,378	0	0	2,465	2,919
Nov.	0,211	0	0		0,969	0,208	0,409	0,652	2,449
Dec.	0	0	0	0,030	0	0	0	0,546	0,576
Jan.	0,079	0	0,091		0,075	0,031	0,447	0,444	1,167
Feb.	0	0,089	0		0	0,093	0,291	3,037	3,510
	0,327	0,251	0,168	1,907	2,391	0,944	2,148	18,975	27,111
	11+	9+	12—	82+	48	22—	23—	100	

For Trial of the Moon's fluence

	New.	1st Qr.	Full.	Last Qr.			Corrected by exc. & flon.
	D. H.	D. H.	D. H.	D. H.			
Mar.	1 10	8 16	16 20	24 2 New. 30 21	2 4 8 10 13 19 20 26 31	9 5 6	6
	1st Qr.	Full.	Last Qr.	New.			
Apr.	7 11 15	10 22	8 29	8 16 17 19 22 24 30		6 4 0	0
May	7 6 14 20 21	14 28	21 28	21 1 6 8 13 20 27		6 1 2	2
June	6 0 13 5	19 20	27 10	7 8 11 12 25 29		6 0 2	2
July	5 14 12 12	19 4	27 1	4 6 16 23 27 28 30		7 4 7	7
Aug.	4 2 10 19	17 14	25 17	1 5 9 14 16 18 22 27		8 5 7	7
Sept	3 12 0 2	16 1	24 0	5 7 16 18 20 22		6 2 2	2

From this table it appears, that, of 92 changes of weather in the whole year, 46 fell on the days of the Moon's pretended influence. And rejecting of these changes, all that were reversed within 24 hours, of 53 that remain in all, 27 fell on the days of lunar influence. And if from these again we reject the octantal days, confining the Moon's influence to the days of syzygie and quadrature, there still remain 14 of the 53 for these days.

Of the new Moons four only were attended with a change of weather, and of the full Moons three; namely, the new Moons of the months of March, July, December, and February; and the full Moons of October, November, January. Both the setting-in and the breaking of the great frost happened on days exempt from Lunar influence.

Upon the whole, the trial turns out more in favour of the Moon this year than it did the last. But still the changes were many more on the days confessedly exempt from her influence, than on those which have been supposed to be the most subject to it.

The greatest monthly height of the barometer was only four times in this year accompanied with a N.E. wind, namely, in the months of April, June, November, and February. It was five times attended with a S.W. namely, in March, May, August, September, and December; and the greatest height observed in the whole year was one of these, namely, in the month of March.

Once it was accompanied with the East wind, namely, in January; and twice with a N.W. namely, in July and October.

The least monthly height was once accompanied with a N.E. namely, in January; six times with a S.W. namely, in April, May, August, October, December, and February; twice with the South wind, namely, in March and September; once with the North, namely, in July; once with a S.E. in June; and once with a N.W. in November.

I subjoin a general view of the winds and rain in the two months of January and February 1775, which are not included in the preceding tables.

1775	N	S	E	W	NW	SE	NE	SW	Rain.	
Jan.	0	0	2	2	10	3½	4	4½	1,724	{ 5 days missed in the journal. 1 day missed.
Feb.	0½	1½	0	1	4	1½	1½	17	1,928	
	0½	1½	2	3	14	5	5½	21½		

Comparing this with the table p. 168. of the last volume, I find the general state of the winds and rain for the twelve months, beginning with March 1775, as follows:

N	S	E	W	NW	SE	NE	SW
23	21	16½	22½	51½	32½	72	119

R A I N.

Quarterly rain.		Half-yearly rain.		Year's rain.
Spring,	4,435	Summer,	12,486	24,662
Summer,	8,051			
Autumn,	6,718	Winter,	12,176	
Winter,	5,458			

But note, that in the space of these twelve months
~~seven days in all~~ were missed in the journal.

XIX. *Extract of a Meteorological Journal for the Year 1775, kept at Bristol, by Samuel Farr, M. D. .*

R. June 27, 1776.

Months.	Barometer.			
	Highest.	Lowest.	Mean.	Vicissitude.
January	30,13.	29,08	29,70	81-2
February	30,29	28,74	29,72	113-4
March	30,38	28,75	29,69	73-2
April	30,22	29,42	29,67	41-2
May	30,20	29,63	29,93	55-2
June	30,09	29,40	29,72	40-2
July	29,96	29,50	29,68	26- $\frac{1}{2}$
August	29,89	29,30	29,61	30-1
September	29,93	29,30	29,53	33-1
October	30,13	28,90	29,70	42- $\frac{1}{2}$
November	30,22	28,90	29,50	95-2
December	30,36	28,49	29,85	46-1

An abridged Table of the WINDS for BRISTOL, for the
Year 1775.

	N	E	W	S	NW	SE	NE	SW	Rain.	Frosty Days.	Thunder, &c.
Jan.	0	0	0	$\frac{1}{2}$	$\frac{1}{2}$	6	$4\frac{1}{2}$	$18\frac{1}{2}$	4,529	2	30. S.W.
Feb.	0	0	$\frac{1}{2}$	$\frac{1}{2}$	$4\frac{1}{2}$	4	$1\frac{1}{2}$	18	4,145	2	12. S.W.
Mar.	$\frac{1}{2}$	0	$\frac{1}{2}$	0	8	1	4	16	2,834	1	
Apr.	0	0	1	$\frac{1}{2}$	8	4	$6\frac{1}{2}$	10	0,616	0	29 SE.
May	$\frac{1}{2}$	0	$\frac{1}{2}$	0	10	$\frac{1}{2}$	10	$9\frac{1}{2}$	0,332	0	
June	$\frac{1}{2}$	0	$\frac{1}{2}$	0	$\frac{1}{2}$	$5\frac{1}{2}$	12	11	4,288	0	{ 6. 9. 14. N.E. 11. 28 29. S.E.
July	0	0	0	$\frac{1}{2}$	5	$2\frac{1}{2}$	2	21	5,414	0	27. S.W.
Aug.	0	0	$\frac{1}{2}$	$\frac{1}{2}$	1	4	1	24	6,947	0	4, 5. S.W.
Sept.	0	0	0	2	2	7	$3\frac{1}{2}$	$15\frac{1}{2}$	2,936	4	{ Earthquake 8. S.W. Br 29,30 20. S.W.
Oct.	0	$\frac{1}{2}$	1	1	7	$1\frac{1}{2}$	5	15	3,358	1	19. with hail, S.W.
Nov.	0	$\frac{1}{2}$	0	1	$3\frac{1}{2}$	$9\frac{1}{2}$	10	$5\frac{1}{2}$	2,044	3	17 S.
Dec.	0	$\frac{1}{2}$	$\frac{1}{2}$	0	2.	8	8	12	1,154	12	23. S.W.
Total	$1\frac{1}{2}$	$1\frac{1}{2}$	6	$6\frac{1}{2}$	53	$53\frac{1}{2}$	67	176	38,597	25	

WEATHER FOR THE YEAR 1775.

January. It froze hard the 1st; after that was wet till 19th; snowed on 20th; then rained to 24th, when it froze; snowed again 21st; was rainy to the 30th, when there was a thunder storm at night; 31st fair.

February. Wet till the 14th; fair, with light showers, to 23d; that rainy; the rest of the month fair.

March. Cloudy to 4th; then rainy to 17th; dry to 5th; 26th stormy; 27th fair; on 28th snow; 29th fair; 30th snow and frost at night.

April.

April. A little rain on 4th, 14th, 15th, 18th, 21st, and 30th; the rest fair, and after 21st remarkably warm; lightned on 29th.

May. Fair to the 5th; rainy then and on 17th and 19th; the rest quite fair.

June. Fair to the 5th; stormy, with frequent thunder, to 11th; fair to 14th; after this fair to 20th; wet to the 25th; 26th fair; 27th cloudy; a hard storm on 28th and 29th; 30th fair.

July. Wet and stormy to 14th; after this fair to 24th; that and 25th stormy; 26th fair; a storm on 27th; rain 28th, 29th; 30th and 31st fair.

August. Wet and stormy to 6th; that with 7th and 8th fair; 9th stormy; 10th fair; then stormy to 22d; fair to 26th; and then to the end stormy.

September. 1st and 2d wet; the 3d it froze and was dry to the 5th; after this stormy to 14th; and then to 20th; fair to 26th, when there was a storm. The rest of the month was dry, but cloudy.

October. Fair, with a slight storm, on 4th to 8th; it rained then and on 9th; none till the 14th; wet to 22d; then fair to 31st, which was wet.

November. Dry to the 4th; after that wet; 8th fair; 9th frosty; 10th fair; wet to 19th; fair to 21st; frost 19th and 24th.

December. Wet to the 5th; fair and after 8th frosty to 13th; rainy to 17th; then frosty again to 20th; after this wet to 25th; dry and frosty to 29th; then frosty on 30th, and wet on 31st.

XX. *Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1775. By Thomas Barker, Esquire. Communicated by Sir John Pringle, Bart. P. R. S.*

R. June 27, 1776.

		Barometer.			Thermometer.						Rain.
		Higheft	Loweft	Mean.	In the Houfe.			Abroad.			
					High.	Low.	Mean	High	Low.	Mean	
Jan.	Morn.	29,91	28,72	29,33	47	30	40½	50	20	36½	1,973
	Aftern.				48	31	41½	52½	26	41	
Feb.	Morn.	29,91	28,35	29,24	48	39½	44	49	31½	39	2,522
	Aftern.				49	41	45	51½	36	46	
Mar.	Morn.	30,09	28,61	29,32	48	38	44	46½	28	36½	1,728
	Aftern.				49½	39½	45	54	34	46½	
Apr.	Morn.	29,97	29,05	29,60	64½	40½	49	55	36	44	1,035
	Aftern.				67	42½	51	80	47	55½	
May	Morn.	29,94	29,31	29,67	62	49½	55½	58½	36	49	0,900
	Aftern.				64	50½	57	73	53	61	
June	Morn.	29,87	29,17	29,49	66½	58	62	62	50	56	0,887
	Aftern.				68	59	64	78	59	69½	
July	Morn.	29,71	29,16	29,41	66½	58½	63	63	52	58	4,078
	Aftern.				68	60	64½	78	58½	70	
Aug.	Morn.	29,60	28,98	29,37	65	58½	62	61	48½	54½	4,760
	Aftern.				66	60	63	72	53	65	
Sept.	Morn.	29,67	29,02	29,31	64½	55½	60	60	45½	52½	5,670
	Aftern.				65½	56½	61	71	53	63	
Oct.	Morn.	29,80	28,50	29,38	59½	43½	51½	57½	30	43	3,480
	Aftern.				59½	45	52½	65	39	52	
Nov.	Morn.	29,96	28,50	29,34	48	39	42½	52	26½	36	3,570
	Aftern.				50	39	43	56	34	41½	
Dec.	Morn.	30,06	28,15	29,54	51	35½	42	52	24½	35	1,096
	Aftern.				51½	35½	42	55½	32	40	
31,699											

In

In four years, 1740, 41, 42, and 43, there came but
in.
66,361 of rain. In the last four years 1772, 73, 74,
in.
and 75, there was 124,957, which is nearly twice as
much.

The proportion that the mean months bear to the whole
years at several periods.

	1736-40	41-50	51-60	61-70	71-75	36-75
January	.054	.076	.078	.069	.070	.073
February	.051	.046	.052	.074	.073	.061
March	.047	.074	.066	.049	.058	.061
April	.057	.075	.086	.056	.035	.065
May	.075	.064	.073	.071	.094	.074
June	.075	.123	.097	.112	.079	.101
July	.139	.111	.134	.107	.072	.111
August	.163	.059	.122	.099	.111	.105
September	.113	.095	.062	.074	.156	.092
October	.081	.094	.071	.115	.102	.093
November	.052	.105	.073	.100	.084	.086
December	.093	.078	.086	.074	.066	.078
	1.000	1.000	1.000	1.000	1.000	1.000

The year began favourable, the winter was mild and
not in general wet; there was indeed a pretty deal of
rain the first half of February, but the latter part of
that month was warm and forwarding, and the spring
continued to advance from that time with much fewer
frosty mornings and N.E. winds than there frequently

are at that season, the many strong Westerly winds keeping them back. The seed-time was fine, and the season good for corn. There were Northerly winds the former part of April, but they were not sharp ones; and the latter part of the month was hot, some days more so than in the height of summer.

The former part of the summer was fine, hot, and dry; some stony parishes burnt a good deal, especially where the grounds were hard-stocked, and the crop of hay was but was small; yet in general the grass had got so forward in spring that it held out pretty well. There was a great deal of fine weather this year; and though there was a great deal of rain in the latter part of the summer, so much fine weather was intermixed with it that most of the hay and harvest were got in well. These rains began the beginning of July, were considerable but not frequent at first, came oftener toward the end of it and in August, and were almost continual the first three weeks of September, with several thunder-storms. What harvest was still out, which in this country was chiefly pease and beans, was much spoiled; but in the fens and several other countries a good of barley was not finished. The latter end of September and beginning of October were fine, and finished the harvest; but the rains returned again, and continued to the end of November, yet in less quantities than before, and the wheat seed-time was pretty good. The end of the year was fine and in general dry; at first warm, and afterward frequent frosty mornings, but no settled frost. The dry weather

5

before

before Midsummer suited the wheat and barley, which were this year a good crop, and the grain large and fine, and cheaper than they have been for several years past.

The weather was less favourable in the South of England; the dry spring was drier and more burning; the barley of two growths, and some did not come up till Midsummer. The wet afterward was also greater, especially in Hampshire, so that their hay and harvest suffered more than ours, and their barley, in particular, coming up late, was late ripe, and was half, or in some places most of it, damaged by the wet. The barley failed also in Norfolk, it not earing well on account of the dry season.

For a good many years past, since the seasons have been in general wet, the nature of East winds has been very different from what it was before. Several years after the great frost in 1740 there were a great many N.E. winds in spring, but they were in general cold and dry, stopping vegetation; but for the last ten years, the East winds have been often very wet; many of the greatest summer floods were by rain out of that quarter, and many times there came rain almost as certainly as the wind turned East.

An experiment of parting fresh-water from salt by freezing.

IN the severe frost last January, some salt-water, being set abroad, froze into an ice which was not solid but porous, the hollows being filled with the saltiest part of the water, for the ice, when drained, was quite fresh.

The falt-water, being again fet abroad, froze as before; what remained still unfrozen was now become exceeding falt, but the ice, drained and diffolved, was little if at all brackish. This agrees with what Captain cook mentions in his late voyage, that in $61^{\circ} 35'$ South latitude they filled their water casks with fresh-water, melted out of ice found floating in the sea. By this experiment, if another time more fully repeated, it may be found to what degree the faltnefs of water may be increafed, by continuing to freeze away the fresh-water.

May not the knowledge of this be of ufe to the falt-makers, efpecially in cold countries? The Sun is ftrong enough of itfelf between the Tropics to dry away the fea-water into falt; and, I think, at the falt-works near Lymington, they increafe the faltnefs of the fea-water by drying it away in the Sun before they boil it into falt. And this feems to be another means of parting fresh-water from the falt, which would fave expence in boiling it away, and may be of ufe in the cold countries, and in winter.

XXI. *An Account of the Meteorological Instruments used at the Royal Society's House. By the Hon. Henry Cavendish, F. R. S.*

R. March 14, 1776.

Of the thermometers, with reflections concerning some precautions necessary to be used in making experiments with those instruments, and in adjusting their fixed points.

THE thermometers are both adjusted to FAHRENHEIT'S scale: that without doors is placed out of a two-pair-of-stairs window, looking to the North, and stands about two or three inches from the wall, that it may be the more exposed to the air, and the less affected by the heat and cold of the house. The situation is tolerably airy, as neither the buildings opposite to it, nor those on each side, are elevated above it in an angle of more than 12° ; but as the opposite building is only twenty-five feet distant, perhaps the heat may be a little increased at the time of the afternoon observation by the reflection from thence. In the middle of summer the Sun shines on the wall of the house, against which the thermometer is fixed, for an hour or two before the morning observation, but
never

never shines on the thermometer itself, or that part of the wall close to it, except in the afternoon, long after the time of observing. On the whole, the situation is not altogether such as could be wished, but is the best the house afforded.

The thermometer within doors is intended chiefly for correcting the heights of the barometer, and is therefore placed close to it. The room in which it is kept looks to the North, and has sometimes a fire in it, but not often.

It has been too common a custom, both in making experiments with thermometers and in adjusting their fixed points, to pay no regard to the heat of that part of the quicksilver which is contained in the tube, though this is a circumstance which ought by no means to be disregarded; for a thermometer, dipped into a liquor of the heat of boiling water, will stand at least 2° higher, if it is immersed to such a depth that the quicksilver in the tube is heated to the same degree as that in the ball, than if it is immersed no lower than the freezing point, and the rest of the tube is not much warmer than the air. The only accurate method is, to take care that all parts of the quicksilver should be heated equally. For this reason, in trying the heat of liquors much hotter or colder than the air, the thermometer ought, if possible, to be immersed almost as far as to the top of the column of quicksilver in the tube. As this, however, would frequently be attended with great inconvenience, the observer will often be obliged to content himself with immersing it to a much less depth; but then, as the quicksilver in a great part of
the

the tube will be of a different heat from that in the ball, it will be necessary to apply a correction on that account to the heat shewn by the thermometer; to facilitate which the following table is given, in which the upper horizontal line is the length of the column of quicksilver contained in that part of the tube which is not immersed in the liquor expressed in degrees; the first perpendicular column is the supposed difference of heat of the quicksilver in that part of the tube and in the ball; and the corresponding numbers in the table shew how much higher or lower the thermometer stands than it ought to do. The foundation on which the table is computed is, that quicksilver expands one 11500th part of its bulk by each degree of heat.

Diff. of Heat	Degrees not immersed in the liquors.														
	50	100	150	200	250	300	350	400	450	500	550	600	650	700	750
50	1,2	1,4	1,7	1,9	1,1	1,3	1,5	1,7	2	2,2	2,4	2,6	2,8	3,1	3,3
100	4,1	1,9	1,3	1,8	2,2	2,6	3,0	3,5	3,9	4,4	4,8	5,2	5,7	6,1	6,6
150	7,1	1,3	2,0	2,6	3,3	3,8	4,6	5,2	5,9	6,5	7,2	7,9	8,4	9,2	9,8
200	9,1	1,8	2,6	3,5	4,4	5,1	6,1	7,0	7,8	8,7	9,6	10	11	12	13
250	1,1	2,2	3,3	4,4	5,5	6,4	7,6	8,7	9,8	11	12	13	14	15	16
300	1,3	2,6	3,8	5,1	6,4	7,7	9,1	10	12	13	14	16	17	18	20
350	1,5	3,0	4,6	6,1	7,6	9,1	11	12	14	15	17	18	20	21	23
400	1,7	3,5	5,2	7,0	8,7	10	12	14	16	17	19	21	23	24	26
450	2	3,9	5,9	7,8	9,8	12	14	16	18	20	22	24	25	27	29
500	2,2	4,4	6,5	8,7	11	13	15	17	20	22	24	26	28	31	33
550	2,4	4,8	7,2	9,6	12	14	17	19	22	24	26	29	31	34	36

But as the generality of observers will be apt to neglect this correction, it would be proper to form

two

two sets of divisions on such thermometers as are intended for trying the heat of liquors; one of which should be used when the tube is immersed almost to the top of the column of quicksilver; and the other, when not much more than the ball is immersed; in which last case the observer should be careful, that the tube should be as little heated by the steam of the liquor as possible. It must be observed, however, that the heat of the liquor may be estimated with much more accuracy by the first set of divisions, with the help of the correction, than it can by the second set, as the latter method is just only in one particular heat of the atmosphere, namely, that to which the divisions are adapted; but, if they are adapted to the mean heat of the climate for which the thermometer is intended, the error can never be very great, and, when the liquor is much hotter or colder than the air of that climate ever is, will be much less than if the first set of divisions were used without any correction; but, when the liquor is within the limits of the heat of the atmosphere, greater accuracy will sometimes be obtained by using the first set of divisions than the second, for which reason the latter set should not be continued within those limits. I would willingly have given rules for the construction of this second set of divisions, but am obliged to omit it, as it cannot be done properly without first determining, by experiment, how much the quicksilver in the tube is heated by immersing the ball in hot liquors.

In a spirit thermometer, the error proceeding from the fluid in the tube being not of the same heat as that in
the

the ball, is much greater; as spirits of wine expand much more by heat than quicksilver: for which reason spirit thermometers are not so proper for trying the heat of liquors as those of quicksilver.

Another circumstance which ought to be attended to in adjusting the boiling point of a thermometer is, that the ball should not be immersed deep in the water; for, if it is, the fluid which surrounds it will be compressed by considerably more than the weight of the atmosphere, and will therefore acquire a sensibly greater heat than it would otherwise do. The most convenient vessel I know for adjusting the boiling point is represented in fig. 1. ABCD is the vessel; AB the cover, made to take on and off readily; E a chimney to carry off the steam; FG the thermometer, passed through a hole *mm* in the cover, and resting in a little bag fastened to the wire HK, intended to prevent the ball from being broken by accidentally falling to the bottom. This wire is made so as to be raised higher or lower at pleasure, and must be placed at such a height that the boiling point shall rise very little above the cover. The hole *mm* is stopped with bits of cork or tow. By this means, as the tube is inclosed in a vessel intirely filled with the steam of boiling water, the quicksilver in it is heated to the same degree as that in the ball; and besides, that part of the tube, on which the boiling point is to be placed, is defended from the vapour, so that it is easy making a mark on the glass with ink. If such a vessel as this is used, the thermometer will be found to stand not sensibly higher

when the water boils vehemently than when it boils gently; and if the mouth of the chimney is covered by any light body, in such manner as to leave no more passage for the steam than what is necessary to prevent the body from being blown off by the pressure of the included vapour, the thermometer will stand only half or three quarters of a degree higher, if the ball is immersed a little way in the water, than if it is exposed only to the steam. But if the covering of the chimney is removed, the thermometer will immediately sink several degrees, when the ball is exposed only to the steam, at least if the cover does not fit close; whereas when the ball is immersed in the water, the removal of the covering has scarce any effect upon it. Whence it appears, that the steam of water boiling in a vessel, from which the air is perfectly excluded, is a little but not much cooler than the water itself, but is considerably so if the air has the least admission to the vessel. Perhaps a still more convenient method of adjusting the boiling point would be not to immerse the ball in the water at all, but to expose it only to the steam, as thereby the trouble of keeping the water in the vessel to the right depth would be avoided; and besides, several thermometers might be adjusted at the same time, which cannot be done with proper accuracy when they are immersed in the water, unless the distance of the boiling point from the ball is nearly the same in all of them. At present there is so little uniformity observed in the manner of adjusting thermometers, that the boiling point, in instruments made by our

best artists, differ from one another by not less than $2\frac{1}{2}^{\circ}$; owing partly to a difference in the height of the barometer at which they were adjusted, and partly to the quicksilver in the tube being more heated in the method used by some persons than in that used by others. It is very much to be wished, therefore, that some means were used to establish an uniform method of proceeding; and there are none which seem more proper, or more likely to be effectual, than that the Royal Society should take it into consideration, and recommend that method of proceeding which shall appear to them to be most expedient.

Of the barometer, rain-gage, wind, and hygrometer.

THE barometer is of the cistern kind, and the height of the quicksilver is estimated by the top of its convex surface, and not by the edge where it touches the glass, the index being properly adapted for that purpose. This manner of observing appears to me more accurate than the other; because if the quicksilver should adhere less to the tube, or be less convex at one time than another, the edge will, in all probability, be more affected by this inequality than the surface. I prefer the cistern to the syphon barometer, because both the trouble of observing and error of observation are less; as in the latter we are liable to an error in observing both legs. Moreover, the quicksilver can hardly fail of settling truer in the former

than in the latter; for the error in the settling of the quicksilver can proceed only from the adhesion of its edge to the sides of the tube; now the latter is affected by the adhesion in two legs, and the former by that in only one: and, besides, as the air has necessarily access to the lower leg of the syphon barometer, the adhesion of the quicksilver in it to the tube will most likely be different, according to the degree of dryness or cleanness of the glass. It is true, as Mr. DE LUC observes, that the cistern barometer does not give the true pressure of the atmosphere; the quicksilver in it being a little depressed on the same principle as in capillary tubes. But this does not appear to me a sufficient reason for rejecting the use of them. It is better, I think, where so much nicety is required, to determine, by experiment, how much the quicksilver is depressed in tubes of a given bore, and to allow accordingly.

By some experiments which have been made on this subject by my father Lord CHARLES CAVENDISH, the depression appears to be as in the following table:

Inside diameter of tube.	Grains of quicksilver in one inch of tube.	Depress. of surface of quicksilver.	Inside diameter.	Grains of quicksilver.	Depress. of surface.	Inside diameter.	Grains of quicksilver.	Depress. of surface.
,6	972	,005	,35	331	,025	,20	108	,067
,5	675	,007	,30	243	,036	,15	61	,092
,4	432	,015	,25	169	,050	,10	27	1,40

The first column is the inside diameter of the tube, expressed in decimals of an inch; the second is the weight of a quantity of quicksilver sufficient to fill one inch in length of it; and the third is the corresponding depression of the convex surface of the quicksilver in a cistern barometer, whose tube is of that size. The reason of giving the second column is, because the easiest way of ascertaining the inside diameter of the tube is, by finding the quantity of quicksilver sufficient to fill a given length of it. It is needless saying, that the part of the tube, whose diameter is to be measured, is that answering to the upper part of the column of quicksilver; and that the table can be of no use but to those only who observe by the convex surface.

In this barometer, the inside diameter of the tube is about $\cdot 25$ of an inch, and consequently the depression is $\cdot 05$; the area of the cistern is near 120 times as great as that of the bore of the tube; so that as the quantity of quicksilver was adjusted when the barometer stood at $29\frac{3}{4}$, the error arising from the alteration of the height of the quicksilver in the cistern can scarce ever amount to so much as $\frac{1}{100}$ th of an inch. As the tube appeared to be well filled, it was thought unnecessary to have the quicksilver boiled in it; but that is certainly the surest way of filling a barometer well.

The principal reason of setting down the mean heat of the thermometer within doors, during each month, in the journal of the weather, is this: suppose that any one desires to find the mean height of the barometer in
any

any month, corrected on account of the heat of the quicksilver in the tube; that is, to find what would have been the mean height, if the quicksilver in the tube had been constantly of a certain given heat. To do this it is sufficient to take the mean height of the barometer, and correct that according to the mean heat of the thermometer; the result will be exactly the same as if each observation had been corrected separately, and a mean of the corrected observations taken. For example, suppose it is desired to find what would have been the mean height of the barometer in the month of August 1775, if the quicksilver during that time had been always at 50 degrees of heat: the mean of the observed heights is 29,86 inches, and the mean heat of the thermometer is 65° or $50 + 15$. The alteration of the height of the barometer by 15° of heat, according to M. DE LUC's rule, is, .047 inches; consequently, the corrected mean height is 29,813.

The vessel which receives the rain is a conical funnel, strengthened at the top by a brass ring, twelve inches in diameter. The sides of the funnel and inner lip of the brass ring are inclined to the horizon, in an angle of above 65° ; and the outer lip in an angle of above 50° (a); which are such degrees of steepness, that there seems no probability either that any rain which falls within the funnel, or on the inner lip of the ring, should dash out,

(a) To make what is here said the more intelligible, there is, in fig. 2. given a vertical section of the funnel, ABC and abc being the brass ring, BA and ba the inner lip, and BC and bc the outer.

or that any which falls on the outer lip should dash into the funnel. This vessel is placed on some flat leads on the top of the Society's House. It can hardly be screened from any rain by the chimnies, as none of them are elevated above it in an angle of more than 25° ; and as it is raised $3\frac{1}{2}$ feet above the roof, there seems no danger of any rain dashing into it by rebounding from the lead.

The strength of the wind is divided in the journal into three degrees; namely, gentle, brisk, and violent or stormy, which are distinguished by the figures 1, 2, and 3. When there is no sensible wind it is distinguished by a cypher.

In the future journals of the weather will be given observations of the hygrometer. The instrument intended to be used is of Mr. SMEATON's construction, and is described in Phil. Transf. vol. LXI. p. 198. It is kept in a wooden case, made so as to exclude the rain, but to leave a free passage for the wind, and placed in the open air, where the Sun scarce ever shines on it. The instrument and case are both a present to the Society from Mr. SMEATON. The hygrometer was last adjusted in Dec. 1775, and as the string has now been in use upwards of five years, it is not likely to want re-adjusting soon.

Of the Variation Compass.

IN this instrument, the box which holds the needle is not fixed, but turns horizontally on a center, and has an index fastened to it, pointing to a divided arch on the brass-

brass frame on which it turns; and the method of observing is to move the box, till a line drawn on it points exactly to the end of the needle; which being done, the angle that the needle makes with the side of the frame is shewn by the index. Fig. 3. is the plan of the instrument; $ABba$ is the brass frame, the sides AB and ab being parallel; Ee is a circular plate fastened thereto, on which $cdde$, the box which holds the needle, turns as on a center; Nn is the needle, the pin on which it vibrates, being fixed in the center of the plate Ee ; Bb is the division on the brass frame; and G the index fastened to the box $cdde$, furnished with a vernier division; the division and vernier being constructed so as to shew the angle which the line Ff makes with AB or ab . The instrument is placed in the meridian by the telescope Mm , the line of collimation of which is parallel to AB , and is pointed to a mark fixed due North of it.

Fig. 4. is a vertical section of the instrument passing along the line Ff ; AB is the brass frame; $cdde$ the box which holds the needle; Ee the circular plate on which it turns; Nn is the needle; P and p are small plates of brass fixed to the ends of it, on each of which is drawn a line serving by way of index. These pieces of brass are raised to such a height that their tops are on a level with the point of the pin on which the needle turns. The use of them is, that it is much easier observing this way than when the lines, serving by way of index, are drawn on the needle itself, as by this means the inconvenience proceeding from one kind of vibration in the needle is avoided.

avoided. *s* and *s* are two brass plates, on each of which is drawn a line to which the index at the end of the needle is to point; there is also a line parallel to these drawn on the bottom of the box; these three lines form the line *Ff* in fig. 3. *R* is a double microscope intended to assist us in judging when the index *p* points exactly to the line *F*, that is, to the line drawn on the plate *s*. It is placed so, that a wire *wvw* in its *focus* appears to coincide with this line; and in observing, the box is moved till the wire appears also to coincide with the index *p*.

The cap in the center of the needle is made to take on and off readily, and to fit on upon either face; so that we may on occasion observe with the under face of the needle uppermost, as is represented in fig. 5. But the regular observations are always made with the needle in its upright position, and by the help of the index *p* only; the intention of the other index and of inverting the needle is, to shew whether the line joining the indices *p* and *p*, or the line *pp* as I shall call it, is parallel to the direction of magnetism in the needle, and thereby to find whether, in the usual method of observing, the index *g* shews the true angle which the direction of magnetism makes with the side *AB*. The way of doing this is as follows; having suffered the needle to settle, the observer moves the box by means of the adjusting screw *T*, till the index *p* coincides with the line *F*, and reads off the angle shewn by the vernier. He then moves the box till the other index *p* coincides with the line *f*, which, as the pin on which the needle is suspended is fixed to the brass

frame, may be done without any danger of altering the position of the needle or making it vibrate, and reads off the angle as before. The mean of these two is the true angle which the line pp makes with the side AB , supposing the division and vernier to be rightly constructed, even though neither the lines pp nor pf should pass through the center of the pin. Having done this, he takes off the cap and inverts the needle, and observes by both indices as before. It is plain, that if the line pp is parallel to the direction of magnetism in the needle, this mean will agree with the former, supposing that the magnetic variation has not altered between the observations. On the other hand, if it is not parallel to the direction of magnetism, but makes the variation appear greater than it ought to do when the needle is upright, it will make it appear as much less when the needle is inverted; so that the mean of the two abovementioned means is the true angle which the direction of magnetism in the needle makes with the side AB ; that is, the true variation of the needle at that time and place, supposing AB to be placed accurately in the meridian. Having thus found the true angle which the direction of magnetism makes with AB , he subtracts that shewn by the index P in the upright position of the needle; the difference is the error of the instrument in the usual manner of observing.

It was by this method that the error of the instrument, at the time of the observations in 1774, was found to be $10'$. For example, by a mean of the observations

vations made on Sept. 5. the variation with the needle, in its upright position, was 21.36 by the South end, and 21.27 by the North; with the needle inverted it was 21.19 by the South end, and 21.29 by the North. The mean of all four is 21.28, which is the true variation at that time and place, and is 8' less than that shewn in the upright position of the needle by the South end, which is the end always used in observing; so that by this day's experiment the error of the instrument appeared to be 8'; but by a mean of the observations of this and two other days it came out 10'. Since that time the needle has been altered; and, at the time of the observations in 1775, the error was so small as to be scarcely sensible.

Great care was taken that the metal, of which this variation compass is composed, should be perfectly free from magnetism. There is a contrivance in it for lifting the needle from off the point, and letting it down gently, to prevent injury in carrying from one room to another. The instrument is constructed nearly on the same plan as some made by the late Dr. KNIGHT. The principal difference is, that in his the pin which carried the needle was not fixed to the lower frame as in this, but to the box; the consequence of which was, that when the needle had settled, and the box was moved to make the index on the needle point to the proper mark, it was again put

(b) The quantity found by taking a mean of all the four numbers is evidently the same as that got by taking a mean of the two first and of the two last, and taking a mean of those two means.

F f f 2

into

into vibration, which caused great trouble to the observer. This inconvenience is intirely removed by the present construction. There is no other material difference, except that of the needle being made to invert, and the addition of the telescope. The contrivance of fixing the pin which carries the needle to the lower frame, is taken from an instrument of Lord CHARLES CAVENDISH; that of making the needle invert I have seen in some compasses made by SISSON.

There is a very common fault in the agate-caps usually made for needles, which is, that they are not hollowed to a regular concave, but have a little projecting part in the center of the hollow; the consequence of which is, that the point of the pin will not always bear against the same part of the agate, and consequently the needle will not always stand horizontal; but sometimes one end will stand highest, and sometimes the other, which causes a difficulty in observing. There is also another inconvenience attends it when the indices of the needle are on a level with the point of the pin, which is of more consequence; namely, that it causes the two indices not to agree, and consequently makes a sensible error, when only one index is made use of, at least in nice observations: but when the lines, serving by way of index, are drawn on the needle itself, and therefore are nearly on a level with its center of gravity, it can cause very little error. The agate cap, which was first made for this instrument, was of this kind; and was so faulty, that, if no better could have been procured, it would have been necessary either to have

drawn the lines serving by way of index on the needle itself, or to have observed by both ends, either of which would have been attended with a considerable increase of trouble to the observer; but Mr. NAIRNE, the artist who made the instrument, has since ground some himself, which are perfectly free from this fault, the concave surface being of an extremely regular shape and well polished, and also of a very small radius of curvature; which is a matter of considerable consequence, as otherwise the point of the pin will not easily slip sufficiently near to the bottom of the hollow.

Care was taken to place the variation compass in a part of the house where it is as little likely to be affected by the attraction of the iron work as in any that could be found. As it seemed, however, to be not intirely out of the reach of the influence of that metal, I took the following method to examine how much it was influenced thereby. The instrument was removed into a large garden belonging to a house in Marlborough Street, distant from the Society's House about one mile and a quarter towards the West, where there seemed no danger of its being affected by any iron-work. Here it ~~was~~ placed exactly in the meridian, and compared for a few days with a very exact compass, placed in an adjoining room, and kept fixed constantly in the same situation. It was then removed back to the Society's House, and compared again with the same compass. The observations were as follow:

Observations made with the Society's instrument in the garden.

Time.		Variation by		Diff.	Time.		Variation by		Diff.
		Society's Instrum.	Compass in room.				Society's Instrum.	Compass in room.	
1775	h /	o /	o /	/	1775	h /	o /	o /	/
July 21	4 48V	21 31	21 33	— 2	July 31	11 4M	21 28	21 32	— 4
	5 0	32	35	— 3		11 20	28	30	— 2
	5 26	30	28	+ 2		11 38	30	30	0
	5 43	31	32	— 1		11 57	29	32	— 3
	5 48	30	30	0		0 13V	29	33	— 4
22	10 45M	33	33	0		0 32	30	31	— 1
	11 2	29	30	— 1		2 24	32	35	— 3
	11 18	31	29	+ 2		2 54	32	31	+ 1
	11 37	31	31	0	Aug. 1	10 34M	26	28	— 2
	11 55	31	33	— 2		3 13V	32	33	— 1
	4 36V	31	32	— 1		4 33	29	29	0
	4 53	27	30	— 3		4 46	29	31	— 2
	5 22	24	26	— 2		5 12	27	29	— 2
	5 54	26	26	0		5 35	27	28	— 1
						5 57	28	30	— 2

The instrument being removed back to the Society's house.

Time.		Variation by		Diff.			Variation by		Diff.
		Society's Instrum.	Compass in room.				Society's Instrum.	Compass in room.	
1775 Aug. 2	h / 1 8V	21 45	21 32	+ 13	1775 Aug. 4	h / 10 50M	21 47	21 33	+ 14
	1 10	44	30	+ 14		11 0	47	34	+ 13
	1 20	46	29	+ 17		11 10	47	35	+ 12
	1 30	47	29	+ 18		11 20	47	35	+ 12
	1 40	47	32	+ 15		11 30	46	35	+ 11
	1 50	47	31	+ 16		11 40	47	34	+ 13
	2 0	47	31	+ 16					

By a mean of the observations, the variation shewn by the compass in the room is 1',3 greater than by the Society's instrument in the garden, and 14',1 less than by the same instrument placed in its proper situation; so that the variation appears to be 15',4 greater in that part of the Society's House where the compass is placed, than in the abovementioned garden; and therefore, as there is no likelihood of its being affected by any iron in the latter place, the needle seems to be drawn aside 15' $\frac{1}{2}$ towards the N.W. by the iron work of the house and adjacent buildings.

On comparing the observations of the two last years together, the variation appears, after allowing for the error of the instrument, to have been 27' greater in 1775 than in 1774; though I have been informed by Dr.

HEBERDEN,

HEBERDEN, who has made observations of this kind for several years past, that the annual alteration of the variation has, in general, been not more than $10'$; and in particular, that the alteration in the last year appears to be only $11\frac{1}{2}'$; so that the great difference observed at the Society's House seems to be owing, not solely to the real alteration in the variation, but partly to some other cause; though what that should be I cannot conceive, unless some change was made in the iron work either of this or the adjoining houses between the two periods; but I do not find that any such change has been made. During the last year, indeed, there have been two large magnets in the house, each consisting of several great bars joined together, being what the late Dr. KNIGHT used for making artificial magnets, and at the time of the observations in 1774 there was only one; but their distance from the compass is above fifty feet: and I am well assured, that in the situation in which they are actually placed, they cannot draw the needle aside more than $3'$, and not more than $15'$, when the line joining their poles is placed in such a direction as to act with most force(c). The single

(c) The principle by which this was determined is, that if a magnet is placed near a variation compass, with its poles equidistant from it, and situated so that each shall act equally oblique to the length of the needle, it can have no tendency to alter the variation; and that the situation in which it alters it most, except when placed nearly North or South of the compass, is when the line joining its poles points almost directly towards the needle. This experiment I tried purposely on the occasion, and found it answer; but, I believe, any one skilled in magnetism would have granted the truth of the position without that precaution.

magnet

magnet in the year 1774 was placed nearly in the same situation and direction that the two were in 1775, so that the difference of their effect in these two years can hardly have been so much as 3'; and therefore, the great apparent alteration of the variation between the two periods cannot have been owing to them. Neither can it have been owing to the fault of the agate cap used in the year 1774, as the error proceeding from thence could hardly be more than 2 or 3'. It is intended that, for the future, the abovementioned magnets shall be kept always in the same situation and direction that they are in at present, and in which they were in 1775.

Of the Dipping-needle.

IN this instrument the ends of the axis of the needle roll on horizontal agate planes, a contrivance being applied, by which the needle is at pleasure lifted off from the planes, and let down on them again, in such manner as to be supported always by the same points of the axis resting on the same parts of the agate planes; and the motion with which it is let down is very gradual and without shake. The general form of the instrument, the size and shape of the needle, and the cross used for balancing it, are the same as in the dipping-needle described in Phil. Transf. vol. LXII. p. 476. It is also made by the same artist Mr. NAIRNE.

It may be seen in the Meteorological Journal, that the dip was observed first with the front of the instrument

to the West, and then to the East; after which the poles of the needle were reversed, and the dip observed both ways as before. The reason of this is, that the mean of the observed dips, in these four situations, differs very little from the truth, though the needle is not well balanced, and even though a great many other errors are committed in the construction of the instrument; provided the needle is made equally magnetical after the poles are reversed as before^(d); and that the difference of the observed dip, in these four situations, is not very great, as will appear from the following considerations.

First, let fig. 7. be a front view of the needle; AB a line parallel to the direction of magnetism therein; and CD a perpendicular thereto, meeting it in the line joining the centers of the cylindrical ends of the axis, or in the axis of motion as we may call it. If the needle was truly balanced, its center of gravity would be in d , the intersection of AB and CD. Suppose now, that the needle is not truly balanced, but that its center of gravity is in g ; draw gn perpendicular to AB, cutting it in n ; and let the parts mn and mg be equal. When the instrument is turned half-way round, so that the contrary face of the needle is presented towards us, the edge ADB, which is now lowest, will become uppermost, and the center of gravity will be in that situation in which the point n now is; therefore, the mean between the forces with which the

(d) It is easy to see whether the needle is made equally magnetical after the poles are reversed as before, by counting the number of vibrations which it makes in a minute.

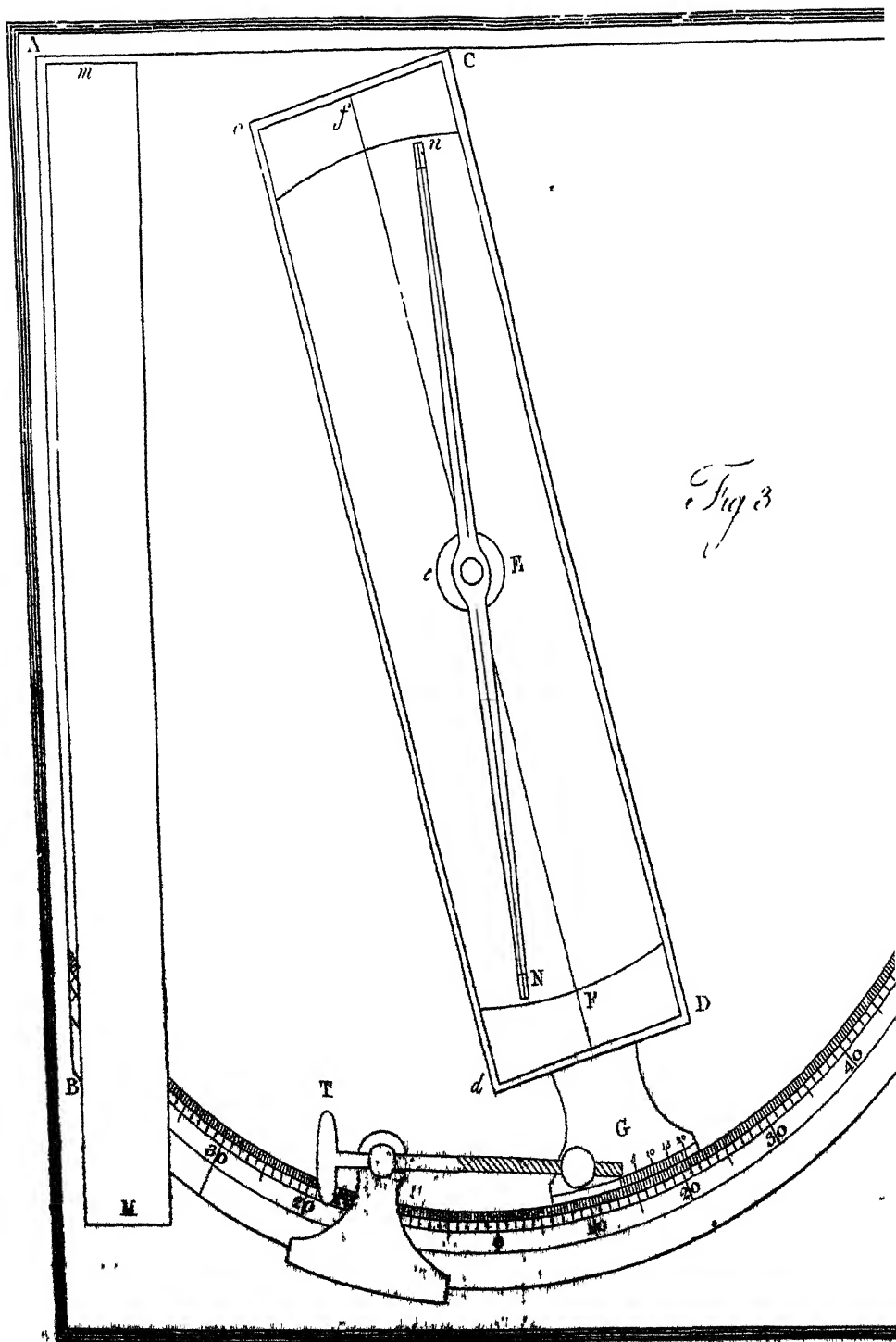
needle is drawn out of its true position in these two situations, in consequence of its not being truly balanced, is accurately the same; and the mean between the two observed dips is very nearly the same, as if the center of gravity was at *m*. But if the center of gravity is at *m*, the dip will be very nearly as much too great in the present state of the needle, as it will be too little when the poles are reversed. Therefore, the mean of the observed dips in these four situations will be very nearly the same as if the needle was truly balanced.

Secondly, if the planes on which the axis rolls are not horizontal, the dip will be very nearly as much greater than it would otherwise be, when one face is turned to the West, as it is less when the other is; for if these planes dip towards the South in one case, they will dip as much towards the North in the other, supposing the levels by which the instrument is set to remain unaltered. Consequently, the mean of the two observations will be very nearly the same as if they were placed truly horizontal.

Thirdly, by the same method of reasoning it appears, that the mean of the two abovementioned observations will be not at all altered, though the line, joining the mark on that end of the needle by which we observe, with the axis of motion, is not parallel to the direction of magnetism in the needle; that is, though the mark does not coincide with the point A or B, or though the line joining the two divisions of 90° is not perpendicular to the horizon, or though the axis of motion does not pass through the center of the divided circle, provided it is in

the same horizontal plane with it. If, indeed, the axis of motion is not in the same horizontal plane with the center of the divided circle, the error proceeding from thence will not be compensated by this method of observing, unless both ends of the needle are made use of. This, however, is of no consequence as, it is easy to examine whether they are in the same horizontal plane or not.

But the error which is most difficult to be avoided is, that which proceeds from the ends of the axis being not truly cylindrical. I before said, that the parts of them which rest on the agate planes are always exactly the same. The instrument is so contrived, however, that we may on occasion, by giving the axis a little liberty in the notches by which it is lifted up and down, make those planes bear against a part of the axis distant about $\frac{1}{100}$ or $\frac{1}{50}$ th of an inch from their usual point of bearing. Now, I find, that when the axis is confined so as to have none of this liberty, and when care is taken, by previously making the needle stand at nearly the right dip, that it shall vibrate in very small arches when let down on the planes; that then, if the needle is lifted up and down any number of times, it will commonly settle exactly at the same point each time, at least the difference is so small as to be scarcely sensible; but if it is not so confined, there will often be a difference of 20' in the dip, according as different parts of the axis rest on the planes, and that though care is taken to free the axis and planes from dust as perfectly as possible, which can be owing only to some irregularity in the axis. Moreover,
if



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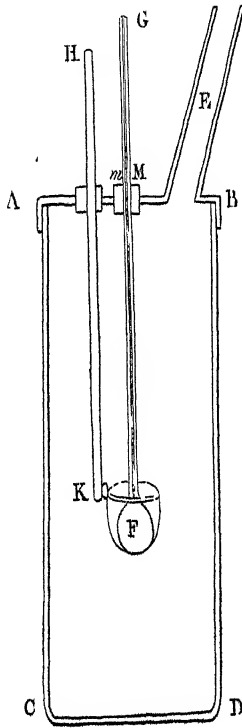


Fig. 1.

Fig. 2.

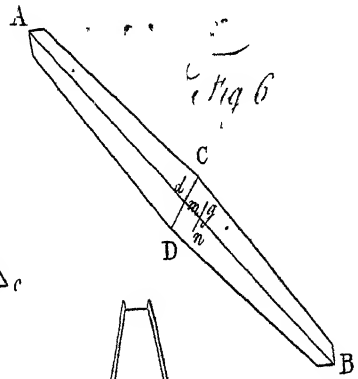
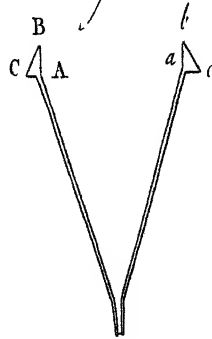


Fig. 6.

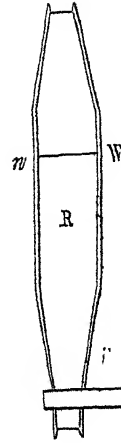


Fig. 4.

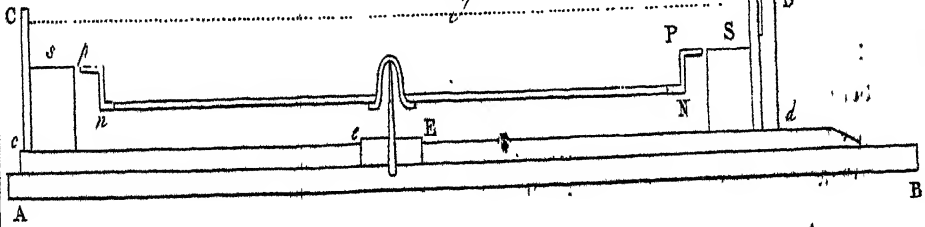
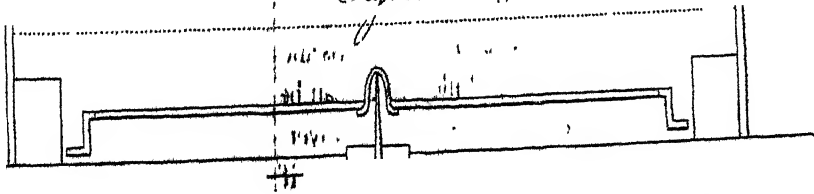


Fig. 5.



if the needle vibrates in arches of five or more degrees, when let down on the planes, there will frequently be as great an error in the dip. It is true, that the part of the agate planes, which the axis rests on when the vibrations are stopped, will be a little different according to the point which the needle stood at before it was let down; which will make a small difference in the dip as shewn by the divided circles, when only one end of the needle is observed, though the real dip or inclination of the needle to the horizon is not altered: but this difference is by much too small to be perceived; so that the above-mentioned error cannot be owing to this cause. Neither does it seem owing to any irregularity in the surface of the agate planes, for they were ground and polished with great accuracy; but it most likely proceeds from the axis flipping in the large vibrations, so as to make the agate planes bear against a different part of it from what they would otherwise do. I have great reason to think, that this irregularity is not owing either to want of care or skill in the execution, but to the unavoidable imperfection of this kind of work. I imagine too, that this instrument is at least as exact, if not more so, than any which has been yet made.

The following table contains the result of some observations which I made, partly with a view to determine the true dip at this time in a place out of reach of the influence of any iron work, and partly to see how nearly different needles would agree. The instruments were all tried in the same garden in which the variation compass was observed, and all on the 10th, 11th, 13th, and 14th days

days of October, 1775, except that marked *, which was tried on the 15th of the preceding April.

			Poles reversed.		Poles restored to their first situation.		True Dip.
	East	West	East	West	East	West	
The Society's needle,	72 32	72 8	72 9	72 40	72 59	71 50	72 23
Another of the same construction, belonging to Mr. NAIRNE,	72 56	72 29	71 45	73 21	72 51	72 27	72 37
One of mine on nearly the same construction,	72 33	72 22	71 41	73 23	72 34	72 18	72 30
Another needle in the same frame,	72 22	72 7	71 40	73 53	72 16	72 30	72 33
A needle of mine, made by Sisson, partly on the same construction as Mr. LORIMER's (e),	* 73 1	71 49	71 57	73 0			72 27
Another of Mr. NAIRNE's on the same construction,	73 8	72 0	73 15	71 57			72 35

Each of the numbers set down in the above table is the mean of two observations, the instruments being observed first with the front to the East, then to the West; then a second time to the East, and then again to the West; and in all the observations, except those with the two last instruments, which are of a different construction, care was taken that the needle should vibrate in very small arches when let down on the agate planes. By a mean of all, the true dip at London, at this time, comes out $72^{\circ} 30'$, the different needles all agreeing

(e) See Phil. Trans. vol. LXV. p. 79.

within $14'$, which is a difference considerably less than I should have expected. It appears also, that the dipping-needle, in the situation in which it is placed at the Society's House, is not much affected by any iron work, as the dip shewn by it in the garden differs only $7'$ from that set down in the journal of the weather.

According to NORMAN, the inventor of the dipping-needle, the dip at London in the year 1576 was $71^{\circ} 50' (e)$; in 1676 it was $73^{\circ} 47'$, according to Mr. BOND (f) ; Mr. WHISTON in 1720 made it $75^{\circ} 10' (g)$; Mr. GRAHAM in 1723 made it between $73\frac{1}{2}$ or $75^{\circ} (h)$, his different trials varying so much; and at present it appears to be $72^{\circ} 30'$. I do not know how much Mr. BOND's determination is to be depended on, as he does not say by what means he arrived at it; but, I believe, Mr. WHISTON's is pretty accurate, for he observed the dip in many parts of the kingdom, and the observations agree well together; so that it is reasonable to suppose, that his instrument was a good one, and that he observed in places where the needle was not much influenced by iron work. The dip, therefore, seems to have been considerably greater about the year 1720, than it was in NORMAN's time, or is at present: it appears, however, to alter very slowly in comparison of the variation.

(e) New Attractive, c. 4.

(f) Longitude found, p. 65.

(g) Longitude and Latitude found by Dipping-needle, p. 7, 49, and 94.

(h) Phil. Trans. N^o 389. p. 332.

XXII. *The Method taken for preserving the Health of the Crew of His Majesty's Ship the Resolution during her late Voyage round the World. By Captain James Cook, F. R. S. Addressed to Sir John Pringle, Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

S I R,

Mile-end,
March 5, 1776.

R. Mar. 7,
1776.

AS many gentlemen have expressed some surprize at the uncommon good state of health which the crew of the *Resolution*, under my command, experienced during her late voyage; I take the liberty to communicate to you the methods that were taken to obtain that end. Much was owing to the extraordinary attention given by the Admiralty, in causing such articles to be put on board, as either by experience or conjecture were judged to tend most to preserve the health of seamen. I shall not trespass upon your time in mentioning all those articles, but confine myself to such as were found the most useful.

We had on board a large quantity of Malt, of which was made sweet-wort, and given (not only to those men who had manifest symptoms of the scurvy, but to such also as were, from circumstances, judged to be most liable to
that

that disorder) from one to two or three pints in the day to each man, or in such proportion as the surgeon thought necessary; which sometimes amounted to three quarts in the twenty-four hours. This is without doubt one of the best antiscorbutic sea-medicines yet found out; and if given in time will, with proper attention to other things, I am persuaded, prevent the scurvy from making any great progress for a considerable time: but I am not altogether of opinion, that it will cure it in an advanced state at sea.

Sour Krout, of which we had also a large provision, is not only a wholesome vegetable food, but, in my judgment, highly antiscorbutic, and spoils not by keeping. A pound of it was served to each man, when at sea, twice a week, or oftener when it was thought necessary.

Portable Soup or Broth, was another essential article, of which we had likewise a liberal supply. An ounce of this to each man, or such other proportion as was thought necessary, was boiled with their pease three days in the week; and when we were in places where fresh vegetables could be procured, it was boiled with them and with wheat or oatmeal, every morning for breakfast, and also with dried pease and fresh vegetables for dinner. It enabled us to make several nourishing and wholesome messes, and was the means of making the people eat a greater quantity of greens than they would have done otherwise.

Further, we were provided with Rob of lemons and oranges; which the surgeon found useful in several cases.

Amongst other articles of victualling we were furnished with sugar in the room of oil, and with wheat instead of much oatmeal, and were certainly gainers by the exchange. Sugar, I imagine, is a very good antiscorbutic; whereas oil, such at least as is usually given to the navy I apprehend has the contrary effect. But the introduction of the most salutary articles, either as provision or medicines, will generally prove unsuccessful, unless supported by certain rules of living.

On this principle, many years experience, together with some hints I had from Sir HUGH PALLISER, the Captains CAMPBELL, WALLIS, and other intelligent officers, enabled me to lay down a plan whereby all was to be conducted. The crew were at three watches, except upon some extraordinary occasions. By this means they were not so much exposed to the weather as if they had been at watch and watch: and they had generally dry cloaths to shift themselves when they happened to get wet. Care was also taken to expose them as little as possible. Proper methods were employed to keep their persons, hammocks, bedding, cloaths, &c. constantly clean and dry. Equal pains were taken to keep the ship clean and dry between decks. Once or twice a week she was aired with fires; and when this could not be done, she was smoaked with gunpowder moistened with vinegar or water. I had also frequently a fire made in an iron pot at the bottom of the well, which greatly purified the air in the lower parts of the ship. To this and cleanliness, as well in the ship as amongst the people, too great attention

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cannot

cannot be paid; the least neglect occasions a putrid, offensive smell below, which nothing but fires will remove: and if these be not used in time, those smells will be attended with bad consequences. Proper care was taken of the ship's coppers, so that they were kept constantly clean. The fat, which boiled out of the salt beef and pork, I never suffered to be given to the people, as is customary; being of opinion that it promotes the scurvy. I never failed to take in water wherever it was to be procured, even when we did not seem to want it; because I look upon fresh water from the shore to be much more wholesome than that which has been kept some time on board. Of this essential article we were never at an allowance, but had always abundance for every necessary purpose. I am convinced, that with plenty of fresh water, and a close attention to cleanliness, a ship's company will seldom be much afflicted with the scurvy, though they should not be provided with any of the antiscorbutics before mentioned. We came to few places where either the art of man or nature did not afford some sort of refreshment or other, either of the animal or vegetable kind. It was my first care to procure what could be met with of either by every means in my power, and to oblige our people to make use thereof, both by my example and authority; but the benefits arising from such refreshments soon became so obvious, that I had little occasion to employ either the one or the other.

These, SIR, were the methods, under the care of Providence, by which the *Resolution* performed a voyage of

three years and eighteen days, through all the climates from 52° North to 71° South, with the loss of one man only by disease, and who died of a complicated and lingering illness, without any mixture of scurvy. Two others were unfortunately drowned, and one killed by a fall; so that of the whole number with which I set out from England I lost only four.

I have the honour to be, SIR, &c.

Extract of a Letter from Captain Cook to Sir John Pringle, dated Plymouth Sound, July 7, 1776.

I ENTIRELY agree with you, that the dearth of the Rob of lemons and of oranges will hinder them from being furnished in large quantities, but I do not think this so necessary; for though they may assist other things, I have no great opinion of them alone. Nor have I a higher opinion of vinegar: my people had it very sparingly during the late voyage; and towards the latter part, none at all; and yet we experienced no ill effects from the want of it. The custom of washing the inside of the ship with vinegar I seldom observed, thinking, that fire and smoke answered the purpose much better.

XXIII. *Extraordinary Electricity of the Atmosphere observed at Iflington on the Month of October, 1775. By Mr. Tiberius Cavallo. Communicated by William Watson, M. D. V. P. R. S.*

R. Mar. 12,
1776. **B**EFORE I enter on the particular narration of the observation made with an electrical kite on the 18th of last October, it will be necessary to give an idea of the scale of my quadrant electrometer used on the occasion, which, being constructed in some measure different from what are commonly sold in shops, will, no doubt, give an unsettled idea of my narration, by expressing the same intensity of electricity under different degrees from the others. In order to this, therefore, it must be observed, that, when the kite is raised, I generally connect with the end of its string a small cylindrical conductor, nine inches long and one inch diameter, made of pasteboard covered with tin-foil; with this I connect the quadrant electrometer, which shews me exactly the state, increase, and decrease of electricity. The apparatus being thus disposed, I have observed, that when the electrometer is at 10° , a little bran presented to the conductor will be attracted by it at the distance of about six-tenths of an inch; when the electrometer is at 20° , the bran will be attracted at the distance of one inch and a quarter; when at 30° , it will
be

be attracted at the distance of two inches and one-fifth; and when at 35° , it will be attracted at the distance of three inches. This being premised, I come now to the narration of the experiment.

October the 18th, after having rained a great deal in the morning and night before, the weather grew a little clear in the afternoon, the clouds appearing separated and pretty well defined; the wind was West, and rather strong, and the atmosphere in a temperate degree of heat. In these circumstances, at three o'clock in the afternoon, I raised a small electrical kite, which measured three feet nine inches in length, and three feet in breadth, giving to it 360 feet of wired string. The angle that the string, or rather the cord of the incurvated string, generally made with the horizon, was near 60° , and, in consequence, the kite's perpendicular height was about 310 feet. After the end of the string had been insulated with a silk lace, and a leathern ball covered with tin-foil had been hanged to it, I tried the power and quality of the electricity, and found it positive and pretty strong; in a little time a small cloud passing over, the electricity increased a little; but the cloud being gone, it decreased again to its former degree. The string of the kite now was fastened by the silk lace to a post in the yard of the house where I live, which is situated near Islington; and I was amusing myself and some other persons with charging two coated phials, and giving several flocks with them. While I was so doing, the electricity, still positive, began to decrease; and in two or three minutes time it was so weak, that it could be
hardly

hardly perceived with a very sensible electrometer, made with two cork balls after Mr. CANTON's manner. Seeing at the same time, that a large and black cloud was approaching the zenith, which, no doubt, caused the decrease of electricity, indicating imminent rain, I introduced the end of a string through a window in a first-floor room, wherein I fastened it by the silk lace to an old chair; the quadrant electrometer was fixed upon the same window, and was connected by a wire with the string of the kite. Being now three quarters of an hour after three o'clock, the electricity was absolutely unperceivable: however, in two or three minutes time it began again to appear, but now, upon trial, was found to be negative; so that it was plain, that its stopping was no more than a change from positive to negative, which was evidently occasioned by the approach of the cloud; part of which by this time had reached the zenith of the kite, and the rain also had begun to fall in large drops. The cloud came farther on, the rain increased, and the electricity keeping pace with it, the electrometer soon arrived to 15° . Seeing now that the electricity was pretty strong, I took again the two coated phials, and began again to charge them, and to give shocks to several by-standers; but the phials were not charged above three or four times, before I perceived that the electrometer was arrived to 35° , and was still increasing. The shocks now being very smart, I desisted from charging the phials any longer; and, considering the rapid advances of the electrometer, thought to take off the insulation of the string,

in

in case, that, if the electricity should increase farther, it might be silently conducted to the earth, without causing any bad accident by being accumulated in the insulated string. To effect this, as I had no proper apparatus near me, I thought to take away the silk lace, and fasten the string itself to the chair; accordingly I disengaged the wire that connected with the electrometer, laid hold of the string, untied it from the silk lace, and fastened it to the chair. But while I effected this, which took up less than half a minute, I received about a dozen or fifteen very hard shocks, which I felt all along my arms, in my breast and legs, shaking me in such a manner, that I had hardly power enough to effect my purpose, and to warn the people in the room to keep their distance. As soon as I took my hand off of the string, the electrical fluid, in consequence of the chair being a bad conductor, began to snap between the string and the shutter of the window, which was the nearest body to it. The snappings, which were audible at a good distance out of the room, seemed at first *isochronous* with the shocks I had received; but in about a minute's time they became more frequent, so that the people of the house compared their sound to the rattling noise of a jack going when the fly is off. The cloud was now just over the kite; it was black and pretty well defined, of almost a circular form, its diameter appearing to be about 40° . The rain was copious, but not remarkably heavy. As the cloud was going off, the electrical snappings began to weaken, and in a short time became un audible. I went then near the string, and finding the electricity weak,

but still negative, I insulated it again, thinking to keep the kite up some time longer; but, as another larger and denser cloud was approaching apace towards the zenith, and I had then no proper apparatus to prevent bad accidents, I resolved to pull the kite in. Accordingly, a gentleman, who was by me, began pulling it in, while I was winding up the string. The other cloud was now very nearly over the kite; and the gentleman who was pulling in the string told me, that he had received one or two slight shocks in his arms; and that, if he were to receive one more, he would certainly let the string go. Upon which I laid hold of the string, and pulled the kite in as fast as I could, without any farther observation. When the kite was pulled in, it was ten minutes after four o'clock; so that all the time that this experiment took up was one hour and ten minutes. There was neither thunder or lightning perceived that day, nor indeed for some days before or afterwards.

XXIV. *Proposals for the Recovery of People apparently drowned.* By John Hunter, Esq. F. R. S.

R. Mar. 21, 1776. **H**AVING been requested by a principal member of the society, lately established for the recovery of persons apparently drowned, to commit my thoughts on that subject to paper; I readily complied with his request, hoping, that, although I have had no opportunities of making actual experiments upon drowned persons, it might be in my power to throw some lights on a subject so closely connected with the inquiries which, for many years, have been my favourite business and amusement. I therefore collected together my observations and experiments relative to the loss and recovery of the actions of life, and shewed them to a Society of which I am a member; who approved of them as new and curious, and unanimously recommended their being submitted to the judgement of this learned Body. The practice is new, and has furnished as yet few important and clear facts. If we judge of the question by our general knowledge of the animal œconomy, I am afraid, it is so imperfectly understood, that our reasoning from it alone could not be relied on: nevertheless, on a subject so interesting to humanity, we must not be idle; we must throw out our observations, and reason as well as we can
from

from the few *data* we have, in hopes that the subject, thus put fairly into the hands of the publick, may in time, by their united endeavours, become perfectly understood.

I shall consider an animal, apparently drowned, as not dead; but that only a suspension of the actions of life has taken place. This, probably, is the case in the beginning of all violent deaths, except those caused by lightning or electricity, by which absolute death may be produced instantaneously.

How a blow on the stomach causes death I have not been able to ascertain. In all those cases which have fallen under my observation, the concomitant circumstances have been such as also attend death caused by electricity; *viz.* a total and instantaneous privation of sense and motion without convulsions, and consequently without any succeeding rigor of muscles, totally differing in these circumstances from death, where the person is struck senseless by any injury done to the brain. I should consider the situation of a person drowned to be similar to that of a person in a trance. In both the action of life is suspended, without the power being destroyed; but I am inclined to believe, that a greater proportion of persons recover from trances than from drowning; because a trance is the natural effect of a disposition in the person to have the action of life suspended for a time; but drowning being produced by violence, the suspension will more frequently last for ever, unless the power of life is roused to action by some applications of art. That I

may more fully explain my ideas upon this subject, it will be necessary to state some propositions.

First, that so long as the animal retains the powers, though deprived of the action, of life, the cause of that privation may frequently be removed; but, when the powers of life are destroyed, the action ceases to be recoverable. Secondly, it is necessary to mention that I consider part of the living principle as inherent in the blood^(a). The last proposition I have to establish is, that the stomach sympathizes with every part of an animal, and that every part sympathizes with the stomach; therefore, whatever acts upon the stomach as a cordial, or rouses its natural and healthy actions, and whatever affects it, so as to produce debility, has an immediate effect upon every part of the body. This sympathy is strongest with the vital parts. Besides this universal sympathy between the stomach and all parts of the body, there are peculiar sympathies; for instance, the heart sympathizes immediately with the lungs. If any thing is received into the lungs, which is a poison to animal life, such as the volatile part in the burning of charcoal, volatile vitriolic acid, and many other well known substances, the motion of the heart immediately ceases, much sooner than if the

(a) That the living principle is inherent in the blood, is a doctrine which the nature of this paper will not allow me at present to discuss; thus much, however, it may be proper to say, that it is founded on the results of many experiments. But it may be thought necessary here, to give a definition of what I call the living principle: so far as I have used that term in this paper, I mean, to express that principle which preserves the body from dissolution with or without action, and is the cause of all its actions.

trachea

trachea had been tied; and from experiments it appears, that any thing salutary to life, applied to the lungs, will restore the heart's action after it has been at rest some time.

I shall divide violent deaths into three kinds. First, where only a stop is put to the action of life in the animal; not, however, by any irreparable injury to a vital part. If this action is not restored in a certain time, it will be irrecoverably lost. The length of that time is subject to considerable variation; which probably depends on circumstances, we are at present unacquainted with. The second is, where an injury is done to a vital part: as, by taking away blood till the powers of action are lost, by a wound or pressure on the brain or spinal marrow; notwithstanding which, there remains sufficient life in the solids, if actions could be restored to the vital parts. The third is, where absolute death instantly takes place in every part, which is often the case in strokes of lightning; in the common method of killing eels, by throwing them on some hard substance, in such manner as that the whole length of the animal shall receive the shock at the same instant; and, as I believe, by a blow on the stomach; in all which cases the muscles remain flexible^(b).

(b) On the other hand, when an eel is killed by chopping it into a number of pieces, the powers of life are by those means roused into action; and, as every part dies in that active state, every part is found stiff after death. This explains the custom of cutting fish into pieces while yet alive, in order to make them hard, usually known by the name of crimping.

Now, the present consideration is, which of the kinds of violent death drowning comes under? I think, it comes under the first; and upon that ground I shall consider the subject.

The loss of motion in drowning seems to arise from the loss of respiration, and the immediate effects which this has upon the other vital motions of the animal; at least, this privation of breathing appears to be the first cause of the heart's motion ceasing; therefore, most probably, the restoration of breathing is all that is necessary to restore the heart's motion: for if a sufficiency of life still exists to produce that effect, we may suppose every part equally ready to move the very instant in which the action of the heart takes place, their actions depending so much upon it. What makes it very probable, that the principal effect depends upon air being thrown into the lungs, is, that children in the birth, when too much time has been spent after the loss of that life which is peculiar to the *fœtus*, lose altogether the disposition for the new life. In such cases there is a total suspension of the actions of life, the child remains to all appearance dead, and would die, if air were not thrown into its lungs, and the first principle of action by these means restored. To put this in a still clearer light, I will give the result of some experiments which I made, in the year 1755, upon a dog.

A pair of double bellows were provided, constructed in such a manner as by one action to throw fresh air into the lungs, and by another to suck out again the air which

had been thrown in by the former, without mixing them together. The muzzle of these bellows was fixed into the *trachea* of a dog, and by working them he was kept perfectly alive. While this artificial breathing was going on, I took off the *sternum* of the dog, and exposed the lungs and heart; the heart continued to act as before, only the frequency of its action was considerably increased. I then stopped the motion of the bellows, and the heart became gradually weaker, and less frequent in its contraction, till it left off moving altogether: by renewing my operation, the heart began again to move, at first very faintly, and with longer intermissions; but, by continuing the artificial breathing, the motion of the heart became again as frequent and as strong as before. This process I repeated upon the same dog ten times, sometimes stopping for five, eight, or ten minutes. I observed, that, every time I left off working the bellows, the heart became extremely turgid with blood, and the blood in the left side became as dark as that in the right; which was not the case when the bellows were working. These situations of the animal appear to me exactly similar to drowning.

The loss of life in drowned people has been accounted for, by supposing that the blood, damaged by want of the action of the air in respiration, is sent, in that vitiated state, to the brain and other vital parts; by which means the nerves lose their effect upon the heart, and the heart in consequence its motion. This, however, I am fully convinced is false: first, from the experiments on the dog,

in whole case a large column of bad blood, *viz.* all that was contained in the heart and pulmonary veins, was pushed forward, without any ill effect being produced; and next, from the recovery of drowned persons and still-born children, which, under those circumstances, never could happen, unless a change of the blood could take place in the brain, prior to the restoration of the heart's motion: therefore, the heart's motion must depend immediately upon the application of such air to the lungs, and not upon the effects which air has upon the blood, and which that blood has upon the vital parts. These are only secondary operations in the animal economy.

It frequently happens in the case of drowning, that assistance cannot be procured till a considerable time after the accident; every moment of which delay renders recovery more precarious, the chances of which are not only diminished in the parts where the first powers of action principally reside, but also in every other part of the body.

In offering my sentiments on the method of treating people who are apparently drowned, I shall lay before you, first, what I would recommend to have done; secondly, what I would wish might be avoided.

When assistance is called in, soon after the immersion, perhaps blowing air into the lungs may be sufficient to effect a recovery (*c*). But if a considerable time, such as

(*c*) Perhaps the dephlogisticated air, described by Dr. PRIESTLEY, may prove more efficacious than common air. It is easily procured, and may be preserved in bottles or bladders.

an hour, has been lost, it is most probable that this will not be sufficient; the heart, in all likelihood, will by this time have lost its nice connection with the lungs. It will, therefore, be proper to apply stimulating medicines, such as the vapour of volatile alkali, mixed with the air; which may easily be done, by holding spirits of harts-horn in a cup under the receiver of the bellows. I would advise the air and volatile alkali to be thrown in by the nose, if by both nostrils so much the better, as we know, that applications of this kind to the olfactory nerves rouse the living principle, and put the muscles of respiration into action, while some applications to the mouth rather depress than rouse, by producing sickness. If during this operation the *larynx* be gently pressed against the *œsophagus* and spine, it will prevent the stomach and intestines being too much distended by the air, and leave room for the application of more effectual *stimuli* to those parts. This pressure, however, must be conducted with judgement and caution, so that the *trachea* and the aperture into the *larynx* may both be left perfectly free. While this business is going on, an assistant should prepare bed-cloaths, carefully brought to the proper degree of heat. I consider heat as congenial with the living principle; increasing the necessity of action it increases action; cold, on the other hand, lessens the necessity, and of course the action is diminished; to a due proportion of heat, therefore, the living principle owes its vigour.

From observations and experiments it appears to be a law of nature in animal bodies, that the degree of heat should bear a proportion to the quantity of life; as life is weakened, this proportion requires great accuracy, while greater powers of life allow it greater latitudes^(d).

I was led to make these observations by attending to persons who are frost-bitten; the effect of cold, in this instance, is that of lessening the living principle. The powers of action remain as perfect as ever, and heat is the only thing wanting to put these powers into action; yet this heat must at first be gradually applied, and proportioned to the quantity of the living principle; but, as life increases, you may increase the degree of heat. If this method is not observed, and too great a degree of heat is at first applied, the person or part loses entirely the living principle, and mortification ensues. This process invariably takes place with regard to men. The same thing, I am convinced, happens to other animals. If an eel, for instance, is exposed to a degree of cold, sufficiently intense to benumb him till the remainder of life in him is scarcely perceptible, keep him still in a cold of about 40° , and this small quantity of life will remain for a considerable time without diminution or increase; but, if he is put into about 60° , the animal will at first shew strong signs of returning life, but will die in a few minutes. Nor is this circumstance peculiar to the

(d) It is upon these principles that cold air is found of so much service to people who are much reduced by disease, as the confluent small-pox, fevers, &c. viz. diminishing heat in proportion to the diminution of life.

diminution of life by cold. The same *phenomena* take place in animals who have been very much reduced by hunger.

If a lizard, or snake, when it goes to its autumnal hiding place, is not sufficiently fat, the living powers become, before the season permits it to come out, very considerably weakened, and perhaps so much as not to be again restored. If those animals, in such a state, are exposed to the Sun's rays, or placed in any situation which, by its warmth, would give vigour to those of the same kind, which are possessed of a larger share of life, they will immediately shew signs of increased life, but quickly sink under the experiment and die; while others, reduced to the same degree of weakness, as far as appearances can discover, will live for many weeks, if kept in a degree of cold proportioned to the quantity of life they possess.

I observed many years ago, in some of the colder parts of this island, that, when intense cold had forced black-birds or thrushes to take shelter in out-houses, any of those that had been caught, and from an ill-judged compassion exposed to a considerable degree of warmth, died very soon. The reason of this I did not then conceive; but I am now satisfied, that it was owing, as in the other instances, to the degree of heat being increased too suddenly for the proportion of life remaining in the animal.

From these facts it appears, that warmth causes a greater exertion of the living powers than cold; and that an animal, in a weakly state, may be obliged by it to exert

a quantity of the action of life sufficient to destroy the very powers themselves. (c) The same effects probably take place even in perfect health. It appears, from experiments made in a hot room, which were read to this Society, that a person in health, exposed to a great degree of heat, found the actions of life accelerated so much as to produce faintness and debility.

If bed-cloaths are put over the person so as scarce to touch him, steam of volatile alkali, or of warm balsams and essential oils, may be thrown in, so as to come in contact with many parts of his body. It will certainly prove advantageous if the same steams can be conveyed into the stomach, as that seat of universal sympathy will by these means be roused. This may be done by a hollow *bougie* and a syringe; but this operation should be performed with all possible nimbleness, because the instrument, by continuing in the mouth, may produce sickness, an effect I should chuse to avoid. Some of the stimulating substances, which are of a warm nature, and have an immediate effect, may be thrown into the stomach in a fluid state; *viz.* spirits of hartshorn, peppermint water, juice of horse-raddish; as many others also, which produce a more lasting *stimulus*, as balsams and turpentine, such as are found to quicken the pulse of a man in health; but the quantity must be small, as they have a tendency to produce sickness. The same steam and substances should also be thrown up by the *anus*.

(e) It is ~~upon~~ this principle that parts mortify in consequence of inflammation.

The

The process recommended under the first head of treatment should still be continued; while that recommended under the second is put in practice, as the last is only an auxiliary to the first. The first, in many cases, may succeed alone; but the second without the first must, I think, always fail in cases where the powers of life are considerably weakened. Motion possibly may be of service, it may at least be tried; but, as it has less effect than any other of the usually prescribed *stimuli*, it should be the last part of the process (*f*). I would recommend the same care to the operator in regulating the proportion of every one of these means as I did before in the application of heat; possibly every one of them may have the same property of destroying entirely the feeble action which they have excited, if administered in too great a quantity; instead, therefore, of increasing and hastening the operations on the first signs of returning life being observed, as usually done, I should wish them to be lessened, that their increase afterwards may be graduated, as nearly as possible, by the quantity of powers as they arise. If the heart begins to move, I would lessen my application of air to the lungs, and with great attention observe when the muscles of respiration began to act, that, when they do, I might leave a great deal to them. I would by

(*f*) How far electricity may be of service, I know not; but it may, however, be tried, when every other method has failed. I have not mentioned injecting stimulating substances directly into the veins, though it might be supposed a proper expedient; because, in looking over my experiments on that subject, I found none where animal life received increase.

all means forbid bleeding, which, I think, weakens the animal principle, and lessens life itself, consequently lessens both the powers and dispositions to action. I would avoid introducing any thing into the stomach, which ordinarily produces *nausea* or vomiting, as that also will have a similar effect. I would avoid likewise throwing any thing in by the *anus*, which might tend to an evacuation that way, as every such evacuation also tends to lessen the animal powers; of course, I have avoided speaking of the fumes of tobacco, which always produce sickness or purging, according as they are applied.

Whoever is appointed by the society for the purposes of recovering drowned people, should have an assistant, well acquainted with the methods intended to be made use of; that, while the one is going on with the first and most simple methods, the other may be preparing other means, so that no time may be lost between the operations; and the more so, as the first will, in all cases, assist the second, and both together may often be attended with success, though each separately might have failed.

A proper apparatus also is as essentially necessary to the institution, a description of which I here annex. First, a pair of bellows, so contrived with two separate cavities, that by opening them, when applied to the nostrils or mouth of a patient, one cavity will be filled with the common air, and the other with air sucked out from the lungs, and by shutting them again, the common air will be thrown into the lungs, and that sucked

out of the lungs discharged into the room. The pipe of these should be flexible, in length a foot or a foot and a half, and at least three-eighths of an inch in width; by this the artificial breathing may be continued, while the other operations, the application of the *stimuli* to the stomach excepted, are going on, which could not conveniently be done, if the muzzle of the bellows were introduced into the nose. The end next the nose should be double, and applied to both nostrils. Secondly, a syringe, with a hollow *bougie*, or flexible *catheter*, of sufficient length to go into the stomach, and convey any stimulating matter into it, without affecting the lungs. Thirdly, a pair of small bellows, such as are commonly used in throwing fumes of tobacco up the *anus*.

I shall conclude this paper by proposing, that all, who are employed in this practice, be particularly required to keep accurate journals of the means used, and the degrees of success attending them; whence we may be furnished with facts sufficient to enable us to draw conclusions, on which a certain practice may hereafter be established.

XXV. *An extraordinary Cure of wounded Intestines.* By
Charles Nourse, *Surgeon, at Oxford.*

TO GEORGE SCOTT, ESQ. F. R. S.

SIR,

Oxford,
Dec. 20, 1775.

R. Mar. 29, 1776. **I**N compliance with your request, and in discharge of the promise I made to you, when I had last the pleasure of seeing you in Oxford, I transmit to you, the case of JAMES LANGFORD, the unfortunate lad after whose welfare you were pleased, out of your great humanity, often to enquire. If any hints may be drawn from this narrative, that may prove useful to the young practitioner, it will not fail to give me much pleasure; if not, I shall always think that time has been well bestowed which has been employed in executing your command. Should the account I have given of this singular case prove worthy the notice of your learned Society, I shall esteem it as my greatest honour.

I am, &c.

THE CASE OF JAMES LANGFORD^(a).

IN the evening of the 26th of September, 1775, I was called, in great haste, to JAMES LANGFORD, a young man in the twenty-first year of his age, who had been maliciously stabbed, with a knife, in the left side of his belly. The wound was between two and three inches in length, running from the left *os ilium* obliquely upwards towards the navel. I found him lying on the floor, weltering in his blood, with a large portion of his intestines forced through the wound; and I learnt, from the unhappy youth himself, that, as soon as the wound was inflicted, the bowels began to appear; and, by the time I got to him, which could not exceed ten minutes, I verily believe, that the full half of the intestinal tube was protruded through the opening. This I attributed, in some measure, to the fulness of the stomach; for, immediately before the accident happened, he had eaten a very hearty supper. The wound at first bled freely; but the hæmorrhage was soon restrained by the pressure of the prolapsed intestines, which were, to a great degree, distended with air; and from this circumstance I was flattered with the best hopes that they had escaped the assassin's knife; but, to my great disappointment, it proved otherwise, as will appear most evidently from the sequel of this narrative. Examining his pulse, I found it

(a) He is a journeyman book-binder, and lives with Mr. ALEXANDER THOMPSON, in Oxford.

was exceedingly low, quick, and interrupted; his skin was all over cold and clammy, and he laboured under great languor, anxiety, and pain about the *præcordia*. He likewise complained of a disagreeable tingling and numbness of the whole thigh, leg, and foot, of the side wounded; and acquainted me, that he dropped on the floor in consequence of the inability of the limb to support him, and not from any faintness, as might have been reasonably expected from the loss of blood, or through fear, to which, indeed, he seemed an utter stranger. I ordered him to be conveyed to his bed in an horizontal posture, lest the raising of the body might encourage a farther descent of the parts which still remained in the *abdomen*; and a fomentation of port wine with warm water to be got ready immediately, out of which a double flannel should be wrung, and applied directly to the prolapsed intestines, and renewed occasionally, to prevent them from getting too dry, as well as to preserve, as much as possible, their natural heat. The reduction of the displaced bowels was begun with laying the patient's legs over an assistant's shoulders, who was desired to kneel upon the bed for that purpose, with his back towards him, and then the legs were brought forward as far as to the hams. By this means the lower parts of the body were elevated, and, in consequence, the weight of the bowels falling back towards the chest, counteracted their further protrusion. While the patient continued in this position, I endeavoured, with my hands, to force the guts back into their proper place; but soon found,

found, from the quantity of them protruded, together with their great inflation, that a larger or more extended pressure than my own hands could afford me was necessary; and not thinking it prudent to employ any of the by-standers in so hazardous a task, lest by their inexperience they might handle the bowels too roughly, I sent for two of my fellow-labourers in the care of the Radcliffe Infirmary, to my assistance. As soon as they came, the reduction was again attempted; one of us directing that portion of the bowel which was last protruded, while the two others made a gentle, regular, and circumscribed pressure from all sides towards the opening. But this endeavour not succeeding, convinced us, that it would be much safer to enlarge the wound, to facilitate the return of the prolapsed parts, than hazard the necessity of handling them too much, or exposing them too long to the external air, either of which would, in all probability, have proved fatal. This being done accordingly, by continuing the wound in the same direction upwards about two inches, the exposed bowels were easily and soon returned into the *abdomen*. We then brought the edges of the wound together, and kept them by the future called *gastrostomia*, leaving a proper opening in the most depending part of it for the discharge of the blood or matter which might be collected in the cavity; and afterwards it was dressed in the usual way, lightly and almost superficially, with an anodyne poultice over all.

The regimen enjoined him, with respect to diet, was only gruel, panado, and sage-tea, with barley water or thin gruel to drink; and the medicines were the following:

℞ *Manna*,

Ol. Amygd. D. ana ℥ ff.

Aq. Alexit. Simp. ℥j.

— *Nucis Moschat.* ℥j. *m. f. haustus quam primum sumendus et quartâ quâque horâ repetendus donec alvus responderit.*

℞ *Decoct. Pectoral.* ℥ viij.

Ol. Lini. ℥ij.

Tinct. Thebaic. guttas xl. f. enema quovis tempore infundendum si dolor abdominis urgeat.

27th, Visiting him early the next morning, I found the night had been spent in great restlessness and inquietude, notwithstanding the clyster had been thrown up according to the direction. He was exceedingly low; his skin felt still cold and clammy; his pulse weak and fluttering; he complained of frequent chills, and an oppressive tightness of his belly, though the wound had discharged considerably a thin, serous humour, which had wetted the bandage quite through. Nor was the tension of the *abdomen*, at this time, sufficient to account for this oppressive pain he complained of; from whence I concluded it to be spasmodic. The dressings were removed; and I desired my apprentice, to foment the part with an infusion of the emollient

emollient flowers for a full hour, and to take particular care that the stupes were applied of a very moderate warmth; often having observed, that this manner of applying them, when an inflammation was either to be resolved or prevented, was more effectual than when the heat has been greater. This observation, upon a little reflection, will be found agreeable to reason; for as great heat proves an astringent, on the contrary, a moderate and kindly warmth relaxes, and, by promoting a free perspiration of the parts to which it is applied, sooner effects the end proposed. The wound was dressed as before, with the addition of two ounces of the species *pro cataplasmate de cymino* to the poultice; and as the draughts he had taken had not produced any motion of the bowels, it was thought proper to inject the under-written clyster, as soon as it could be prepared:

Rx Decoct. pro Clyst. ℥viij.

Ol. Lini. ℥ij.

Elect. Lenitiv.

Mel. Solutiv. ana ℥ss. f. enema.

This in about half an hour occasioned a very copious discharge of *feces*, together with a good deal of blood; some of it congealed into lumps, the rest fluid. This circumstance did not fail to alarm my apprehension of the imminent danger of the lad's situation, as it was no longer to be doubted, but that the bowels were wounded in some part of them; but what part still remained a matter of conjecture. When the clyster had done operating, he took this draught:

Rx Sperm.

℞ *Sperm. Ceti* (*Mucilag. Gum. Arab. Solu.*) ℥j.

Aq. Alexit. Simp. ℥j fl.

Ol. Amygd. D.

Syr. de Meconio, ana ℥ij.

Tinct. Thebaic. guttas x. *M. f. haust.*

Late this evening the fomentation and dressings were again renewed, and directions given, that he should take one of the draughts, with manna, oil, &c. as first prescribed, at three o'clock in the morning; and to repeat them regularly every fourth hour, till they had had their desired effect.

28th, He had got but little sleep in the night, though he had lain something quieter, with short, but interrupted slumbers intervening. His pulse, and all the other symptoms, were much in the same state as yesterday, except a general foreness of the *abdomen*; of which, at this time, he made great complaint, and more particularly about the wounded part. The whole belly was full and tense; and, when I struck it with my finger, it returned an *emphysematose* sound. The discharge from the wound was increased; it had stained the bandage of a deep reddish-brown colour, and was of a disagreeable smell. The draughts he had taken had not yet moved him; therefore, I desired they might be continued, according to the general direction; and that, in case any stools should come off, to put them by, separately, for my inspection. By the time I made my evening visit, he had had two motions; in the first, there was a good deal of fluid blood; with the last, but little, no more than just to give it a tinge.

He

He was evidently relieved by the evacuation; was calmer and more composed; his pulse was rather more up, and his skin warmer. He said, he found himself lithesome; that he was not so tight, and thought he breathed with more freedom. When I came to loosen the bandage, I was greatly surprized to find it daubed all over with the discharge; but, as soon as the dressing was removed, there was no evidence wanting to assure me, that this discharge was in part *fecal*, not only from the colour and smell of it, but likewise from the sharp pain it had occasioned in passing through the wound. My hopes of his recovery now began to fail me; however, I resolved to persevere, and act as though I was sure of success. After dressing, he was ordered to take the anodyne draught, and to begin again the manna draughts with oil early in the morning.

29th, Before I came to visit him, he had had another motion; and the nurse informed me, that his night had been better than any of the preceding ones, he having slept, at different times, full three hours. His pulse was stronger, but remitting, and his skin inclining to perspire. The tongue was foul, and the water clear and pretty high-coloured. In the stool, which had come off this morning, I did not find any blood, or in any he had afterwards during the time of his confinement. The wound had discharged a great deal, and was more inflamed; and the edges of it looked thick and ill-natured, and were ready to separate from each other. The tension of the belly still kept up, though I did not perceive, that it
had

had at all increased. The opening draughts were continued, once in six hours only, through the course of this day, which kept him sufficiently open; and the anodyne was repeated at ten o'clock this night.

30th, This morning things wore but a melancholy aspect. His night had been restless, and his head confused, and he talked sometimes incoherently; his pulse was increased, though exceedingly irregular, and the skin felt hot and dry; he was thirsty, and complained of a great tightness, particularly about the region of the stomach; his countenance was hollow, the eyes being sunk, with a deadness in them not easily to be expressed. The wound had discharged very much, and it was extremely offensive. The edges of it were inverted, much swollen, and separated from each other considerably more than the preceding day. He likewise complained of a sharp, burning pain, deep in the wound, but could not express precisely where. As soon as the wound was dressed, the anodyne clyster was administered; and I desired, he might have a small basin of the infusion of mint, with a knob of fine sugar, got ready for him as soon as possible, and that he would sip it down as warm as he could. At two o'clock this afternoon he was seized suddenly with a most violent vomiting, and brought up a large quantity of bile. This I the more wondered at, as he had never made the least complaint of sickness, or *nausea*, from the time of his accident; for every thing he had taken had sat easy and well upon his stomach. What he had brought up was of so dark a colour, that I
imagined

imagined it was mixed with blood; but, upon a careful examination of it, found I was mistaken. When the vomiting was over, the nurse gave him a little more of the mint infusion; and, soon after, he fell into a sound sleep, which continued more than an hour. In the evening he was hot and uneasy, complaining of thirst, and a pain in his head; his pulse was increased, and his skin felt dry. The wound had made a prodigious discharge, which I observed always to increase, in proportion, as the bowels were more or less loosened by the medicines he was taking; and, from the violent efforts of the *abdominal* muscles in the time of his vomiting, most of the stitches in the wound were broken, so that you might plainly see into the cavity of the *abdomen*. After dressing the wound, twelve ounces of blood were taken from the arm, and the anodyne draught was given to him soon after.

Oct. 1st, I learnt, from the people about him, that for a few hours, after he had taken the *opiate*, he lay composed; but, soon after mid-night, he awaked in great hurry and confusion, complaining of his stomach and bowels, accompanied with convulsive twitchings of the tendons; and that, about five o'clock this morning, he brought up another large quantity of bile, which gave him great relief; for afterwards he lay perfectly easy, and got between two and three hours sleep. At nine o'clock, when I made my morning visit, I found him much refreshed, and without any kind of complaint. His pulse was full, but much steadier than it had been

any time before, and his skin was open. The water he had made was turbid, though still high-coloured. The wound, indeed, made but an indifferent appearance; the edges of it were very floughy, particularly the tendons of the oblique muscle, and so far receded from each other as to make it necessary to divide the remaining fitches. The lower part of the wound, or that next to the *ilium*, was beginning to digest, and the inflammation and tension of the belly to abate. The opening draughts, made a little warmer, were continued, which kept the bowels constantly and gently open. In the evening his pulse was rather increased; and I found that, some time in the afternoon, he had brought up a little more bile, though without any previous complaining. After dressing, I directed more blood to be drawn, and the *opiate* to be repeated.

2d, The nurse acquainted me this morning, that her patient had had a very quiet night, and had slept many hours without intermission; that he had taken a sufficient quantity of nourishment, and that it had sat well on his stomach. I found him chearful, without any complaint, except that of hunger. His pulse was steady, his skin soft and open, his tongue getting cleaner, and his water beginning to break. The discharge, this morning, from the fore was exceedingly offensive; and when I had taken off the dressing, I was really astonished at the horrid appearance! The wound was burst open, in such a manner as to assume a circular form, and was rather more than three inches in the least diameter of it.

it. In the base of this dreadful opening, there was nothing to be seen but the circumvolutions of the small guts; and how this amazing breach was to be restored, I could not easily conceive. Had any one taken a view of the wound at this time, who was unacquainted with the real progress of it, he must naturally have concluded, there had been a great loss of substance. The patient was dressed with thin pledgets of very fine unformed lint, moistened with the oil of the flowers of the *hypericum* luke-warm, laid first upon the exposed bowels; afterwards the cavity was filled up lightly with the same sort of lint dry; the edges were covered with a moderately warm digestive, and the whole secured with the uniting bandage; which bandage had been used from the very beginning, to prevent, as much as art could prevent, the impending mischief.

3d, Appearances this morning were very favourable; he had slept well most part of the night; his pulse was perfectly quiet, and his skin moderately open. The water was become better coloured, and had made a fair separation; so that, from this time, all signs of fever, inflammation, and pain its concomitant, intirely ceased: nor did there even arise any alarming, or even disagreeable symptom afterwards; but every thing went on in an easy, regular way. The wound digested kindly, and was constantly dressed twice a day, as the quantity, and indeed the quality, of the discharge from it required. The opening medicines were repeated occasionally, and his nights secured by a few drops of the Theban tincture.

In a few days, the floughs from the edges of the *abdominal* muscles separated, and left the fore so largely open, that I could easily discover from whence the *feces* made their exit, which was from the middle of that part of the *colon* that lies between the left kidney, to which it is attached, and the upper part of the *sacrum*, where it empties itself into, and forms the *rectum*.

It was exceedingly satisfactory and pleasing to observe, from day to day, the progress Nature made in renovating this formidable breach, and her means of accomplishing it; for, after a little time, the surface of the intestines looked florid, and began to pullulate, throwing up small grains of flesh from every point. These *granules*, daily increasing, united with each other, and after filling up the intervals between the circumvolutions of the bowels, became one uniform surface; which surface meeting with that of the raw edges of the integuments, they both adhered together, and became one continued fore. As the wound incarnated, the *fecal* discharge lessened daily, and about the twenty-second or twenty-third day entirely ceased. I now allowed him chicken broth, milk porridge, calves-feet jelly, &c. The wound was dressed once a day with dry lint only, and in seven weeks it was completely healed.

XXIII. *Extract of a Letter from Mr. Alexander Small, Surgeon to the Train of Artillery at Minorca, to Sir John Pringle, Bart. P. R. S. Dated St. Philip's, Aug. 8, 1775.*

R. Apr 15, 1776. **I** BEG leave to give you this trouble, and to add some conjectures, which may be a kind of addition to Dr. CLEGHORN'S Account of this Island. I live near the Glacis, and the artillery men are constantly lodged in the square within the castle. A little beyond the square is our hospital. My attendance at these two places gives me no more than a salutary exercise. The artificers in the civil branch of the ordnance, who are under my care, live near me in the remains of the town of St. Philip. I call them remains, because many of the houses were destroyed during the late siege, and many since; it being determined, that the town shall be removed to a greater distance from the castle. A new spot is accordingly marked out near English Cove, on the side of the harbour; and barracks for two regiments, together with some houses, are already built. The new as well the old town are built in a very dry situation, on a solid rock, on which there is not a drop of stagnating water, nor is there any near the surface of the earth; for the water the inhabitants have for use, is either rain-water
 2 kept

kept in cisterns, or water drawn out of wells, from twenty to sixty or more feet in depth; nor are there any marshes near either town, or indeed in this part of the island.

The castle of St. Philip stands, or rather is cut out of the solid rock, on a promontory, two-thirds of which are washed by the sea, and is open to the sea winds from two-thirds of the compass. As there is no tide there is no slimy shore, which might send forth putrid vapours at low-water; and if there were a tide, our shore is one continued rock, on which there is not any putrescent substance. Indeed the rocks are so free from filth, that after a strong wind has raised the sea-water, and carried it into cavities hollowed in the rock by storms, it dries there into pure white salt.

During the hot weather in July, August, and September, our unhealthy season, the air is daily ventilated, either by general winds, which pass freely over the island, or by sea-breezes. The air over the land being rarefied by the reflected rays of the Sun, and by being in contact with the heated earth, necessarily makes room for the cooler and denser air in contact with the cooler sea-water. Whence, in such a situation, shall we seek for the causes of *tertianas*, so called here, and so much dreaded during the hot months? Two causes seem to offer themselves; one very obvious, the other rather more remote. The Southerly winds are much complained of here, as occasioning a general lassitude, and as bringing with them noxious *effluvia* from Africa; but whoever considers the
distance

distance between this island and Africa, will scarcely believe, that the air can carry with it, so far, any other quality than the warmth attending the season of the year. Gibraltar, nearer Africa, and more Southerly than we are, is not subject to *tertians*, nor are some places even in this island. The causes therefore must be sought for on the spot. In a situation, such as I have described ours to be, you may believe, that shade and a plentiful supply of fresh succulent culinary plants must be very desirable. On so dry a rock, an artificial supply of moisture must become necessary, especially in a country where there seldom is rain from May to October. It is not an easy matter to keep a due mean in the use of whatever experience shews to be necessary. If a little does good, we are apt to conclude, that a great deal will do more good: thus, I think, it fares with us in regard to the use of water in our gardens. In order to have a garden, it is necessary here to have a draw-well. The drawing of water is the labour of an ass; and, as the labour is not hard, the beast is kept at it pretty constantly, and thus plenty of water is drawn up. As the water is hard, and is much colder than the temperature of the air, it is kept in cisterns for some time, exposed to the Sun, till it acquires the temperature of the air, and thus becomes more friendly to vegetation than if used immediately on being drawn up. Having thus obtained plenty of water, they bestow it most copiously on their gardens. Suppose yourself landed at St. Philip's in this season of the year, on a dry, parched rock, and that you were told, that the rock was uniformly the same all the

way to Mahon, a distance of two miles, and that you were under a necessity of going to Mahon in the evening; would you expect to be serenaded on this rock with the croaking of frogs all the way you went? This literally is the case. The gardens on each side the road are so much watered, that the frogs, bred in the cisterns which contain the water, spread and enjoy themselves around, and frequently take up their abode in trees. This shews that even the trees abound much in watery juices, seeing the exhalations arising from them yield an atmosphere agreeable to the frogs. Where land is thus abundantly watered and closely planted with succulent vegetables, many parts of these vegetables, as well as the insects which feed on them, will be liable to putrify; and a putrid vapour may be thence exhaled in the evening especially, and during the night, when there seldom is wind to carry them off. Wherever the inhabitants can find a proper depth of mould, within a convenient distance of a market, so many sources of putrid exhalations are formed.

Let me give an instance, to shew that this opinion is not merely ideal. On the North-side of St. Philip's there is a road, bounded on the North by a wall, called the Line-wall: Dr. HUCK must remember it and the environs. Along and near that wall there are many gardens, which thus send forth unhealthy vapours; and the effect is, that the houses on the South-side of that road, though facing the North, and thereby, one would think, the healthier, are called Rotten-row, their unhealthiness being

being owing, as is believed, to the vapours arising from these gardens; for houses situated on the South-side of the town, though at a little distance, are by much the more healthy, though more exposed to the heat of the meridian Sun. Other instances might be quoted; and experience has taught us, that it is found to be very prejudicial to health to remain exposed to the evening dews near Mahon or St. Philips, round which these gardens chiefly abound, while country peasants lie in their vineyards whole nights without being hurt, the vines being left to nature for a supply of moisture.

Perhaps it may seem, that, while the heat and drought of the climate makes this method of gardening necessary, and at the same time require a large supply of succulent vegetables and fruits, these ill consequences must be unavoidable. But M. DE CHATEAU-VIEUX, a magistrate of Geneva, has pointed out a very promising remedy, suggested to him by some of his own judicious experiments in agriculture.

The second general cause of *tertians* was pointed out to me by Dr. MUNRO, physician of this island, an ingenious gentleman, and very observant of every thing relating to his profession.

The rocks of this island consist chiefly of two kinds of stone; one so hard that scarce any tool can touch it; and the other so soft, that it is easily cut into any form. It much resembles the Bath stone, and is called Cantoan stone. The first is impervious to any fluid; but the other sucks up, or is penetrated by, moisture, like filtering

stones. I found, some years ago, this to be the case with the Bath stone. Mr. ALLEN covered his workmen's houses with flat stones brought from the quarry, and cut of a proper thickness: I mentioned to one of the inhabitants, that their houses being so closely covered were, I supposed, very warm; and was surprized at being told, that they were much the reverse; for that in rainy weather, the water penetrated through the stones; and that in frosty weather, the inside of the roofs were covered with ice, whereby their upper rooms were of little use to them. These different qualities of the stones in this island are not, perhaps, sufficiently attended to.

When houses are built on the hard rock, all within the walls is levelled; and on that floor the poorer inhabitants live. As this stone takes a greater degree of cold than substances less solid, and does not so soon come to the temperature of the air; it consequently cools, and attracts to it the moisture in the air, and retains it long on its surface. In order to avoid the damp cold feel, if the inhabitants can afford to buy a mat, they cover the floor with it; under which the wet remaining, induces a degree of putrefaction, which renders the houses more unhealthy, and reduces the inhabitants to a state ready to be affected by any distemper, especially by the *tertian*, which spreads by contagion. As the moisture remaining on this stone is but temporary, provided there are drains to carry the water off, its bad effects are easily prevented by keeping a fire burning, or by laying the ground-floor with terrace, or with deal-boards.

When houses are built on the soft Cantoön stone, the rain that falls without soaks through it; and if there are no means of carrying it off, it remains in the stone, becomes putrid, gradually exhales, and thus becomes highly prejudicial to health. I might quote several instances of families dying in consequence of such putrid moisture; but shall rest satisfied with one, because it became an object of general observation.

At a little distance from, and to the Northward of the Line-wall, a lofty building was erected for a house of entertainment. The people who inhabited it became very unhealthy; and in a few years so much so, that two or three whole families died in it. This house, I am told, is built on Cantoön stone, the hollows filled with Cantoön rubbish, and is surrounded by gardens continually watered, some of which are higher than the floor of this building; by which means the stone became the receptacle of the waste water. In order to remedy this inconveniency, the floor was taken up, and a stench arose which the workmen could scarcely bear, and changed the colour of every metallic substance about them. People were impressed with so strong a prejudice against the house, that it remains uninhabited and an useless building. The same has happened in other dwelling-houses; in which the same stench, and other indications of putrefaction, were met with, as in the former case.

I can yet, thank God, say very little of the disease of this country, for we still continue very healthy. I have met with two instances of how fast a hold *tertians* take

of the constitution, when the patients have been subject to frequent relapses. Dr. MUNRO tells me, that an addition of *Myrrh* $\mathfrak{z}\text{ij}$. to *Cort. Peru.* $\mathfrak{z}\text{j}$. is the best medicine he knows in this case. I have tried it in both, with seeming success; for we can say nothing certain on that head till the winter. One of them was much afflicted with pains in the lower part of the belly, even after the fever had left him. I gave him *Calomel.* gr. vi. *Pulv. Rhei* gr. xv. in a bolus, and repeated it, at proper intervals, four times. He thinks his present health is much owing to it; as does my other patient from the same medicine. I have found *calomel* peculiarly beneficial to several children who had periodical heats on them, especially at night.

XXVI. *Of the Tides in the South Seas.* By Captain
James Cook, F. R. S.

TO SIR JOHN PRINGLE, BART. P. R. S.

S I R,

April 2, 1776.
Mile-End,

R. Apr. 18, 1776. **I**N compliance with your request, I send you my observations on the tides in Endeavour River, on the East Coast of New Holland, in latitude $15^{\circ} 26' S$.

About 11 o'clock in the evening of the 10th of June 1770, as we were standing off shore, the ship suddenly struck, and stuck fast on a reef of coral rocks, about six leagues from the land. At this time I judged it was about high water, and that the tides were taking off, or decreasing, as it was three days past the full Moon; two circumstances by no means in our favour. As our efforts to heave her off, before the tide fell, proved ineffectual, we began to lighten her, by throwing over-board our guns, ballast, &c. in hopes of floating her the next high-water; but, to our great surprize, the tide did not rise high enough to accomplish this by near two feet. We had now no hopes but from the tide at midnight; and these only founded on a notion, very general indeed among seamen,

seamen, but not confirmed by any thing which had yet fallen under my observation, that the night-tide rises higher than the day-tide. We prepared, however, for the event, which exceeded our most sanguine expectations; for, about 20 minutes after 10 o'clock in the evening, which was a full hour before high-water, the ship floated. At this time the heads of rocks, which on the preceding tide were, at least, a foot above water, were wholly covered. I was fully satisfied with the truth of the remark, after getting into the river, where we remained from the 17th of June till the 4th of August, repairing the damage the ship had received. As this was to be done with the assistance of the tides, it led me to make the following observations, which upon any other less important occasion might have escaped my notice.

The times of high-water on the full and change days I found to be about a quarter after nine; the evening-tide, at the height of the spring, to rise nine feet perpendicular, the morning-tide scarce seven; and the low-water preceding the highest or evening-tide, to fall or recede considerably lower than the one preceding the morning-tide. This difference in the rise and fall of the tide was uniformly the same on each of the three springs which happened while we lay in the place, and was apparent for about six or seven days; that is, for about three days before and after the full or change of the Moon. During the neep, the tide was very inconsiderable, and if there was any difference between the rise
of

of the tide in the day and in the night, it was not observed; but to the best of my recollection none was perceptible. Excepting two or three mornings, when we had a land-breeze for a few hours, we had the winds from no other direction than S.E., which is the same as this part of the coast, and from which quarter I judged the flood-tide came. The wind, for the most part, blew a brisk gale, and rather stronger during the day than the night. How far this last circumstance might affect the evening-tide, I shall not pretend to determine; nor can I assign any other cause for this difference in the rise and fall of the tide, and therefore must leave it to those who are better versed in this subject than,

SIR, your, &c.

XXVII. *An Experimental Examination of the Quantity and Proportion of Mechanic Power necessary to be employed in giving different Degrees of Velocity to Heavy Bodies from a State of Rest.* By Mr. John Smeaton, F. R. S.

R. April 25,
1776.

ABOUT the year 1686 Sir ISAAC NEWTON first published his *Principia*, and, conformably to the language of mathematicians of those times defined, that “the quantity of motion is the measure of the same, arising from the velocity and quantity of matter conjointly.” Very soon after this publication, the truth or propriety of this definition was disputed by certain philosophers, who contended, that the measure of the quantity of motion should be estimated by taking the quantity of matter and the square of the velocity conjointly. There is nothing more certain, than that from equal impelling powers, acting for equal intervals of time, equal increases of velocity are acquired by given bodies, when unresisted by a medium; thus gravity causes a body, in obeying its impulse during one second of time, to acquire a velocity which would carry it uniformly forward, without any additional impulse, at the rate of 32 ft. 2 in. *per* second; and if gravity is suffered to act upon it for two seconds, it

will have, in that time, acquired a velocity that would carry it, at an uniform rate, juſt double of the former; that is, at the rate of 64 ft. 4 in. *per* ſecond. Now, if in conſequence of this equal increaſe of velocity, in an equal increaſe of time, by the continuance of the ſame impelling power, we define that to be a double quantity of motion, which is generated in a given quantity of matter, by the action of the ſame impelling power for a double time; this will be co-incident with Sir ISAAC NEWTON's definition abovementioned; whereas, in trying experiments upon the total effects of bodies in motion, it appears, that when a body is put in motion, by whatever cauſe, the impreſſion it will make upon an uniformly reſiſting medium, or upon uniformly yielding ſubſtances, will be as the maſs of matter of the moving body, multiplied by the ſquare of its velocity: the queſtion, therefore, properly is, whether thoſe terms, the *quantity of motion*, the *momenta* of bodies in motion, or *forces* of bodies in motion, which have generally been eſteemed ſynonymous, are with the moſt propriety of language to be eſteemed equal, double, or triple, when they have been generated by an equable impuſe, acting for an equal, double, or triple time; or that it ſhould be meaſured by the effects being equal, double, or triple, in overcoming reſiſtances before a body in motion can be ſtopped? For, according as thoſe terms are underſtood in this or that way, it will neceſſarily follow, that the *momenta* of equal bodies will be as the velocities, or as the ſquares of the velocities reſpectively; it being certain, that,

that, whichever we take for the proper definition of the term quantity of motion, by paying a proper regard to the collateral circumstances that attend the application of it, the same conclusion, in point of computation, will result. I should not, therefore, have thought it worth while to trouble the Society upon this subject, had I not found, that not only myself and other practical artists, but also some of the most approved writers, had been liable to fall into errors, in applying these doctrines to practical mechanics, by sometimes forgetting or neglecting the due regard which ought to be had to these collateral circumstances. Some of these errors are not only very considerable in themselves, but also of great consequence to the public, as they tend greatly to mislead the practical artist in works that occur daily, and which often require very great sums of money in their execution. I shall mention the following instances.

DESAGULIERS, in his second volume of *Experimental Philosophy*, treating upon the question concerning the forces of bodies in motion, after taking much pains to shew that the dispute, which had then subsisted fifty years, was a dispute about the meaning of words; and that the same conclusion will be brought out, when things are rightly understood, either upon the old or new opinion, as he distinguishes them; among other things, tells us, that the old and new opinion may be easily reconciled in this instance: that the wheel of an undershot water-mill is capable of doing quadruple work when the velocity of the water is doubled, instead of

double work only; “ because (the adjutage being the
 “ same), says he, we find, that as the water’s velocity is
 “ double, there are twice the number of particles of
 “ water that issue out, and therefore the ladle-board is
 “ struck by twice the matter, which matter moving with
 “ twice the velocity that it had in the first case, the
 “ whole effect must be quadruple, though the instan-
 “ taneous stroke of each particle is increased only in a
 “ simple proportion of the velocity.” See vol. II. Anno-
 tations on lecture 6th, p. 92.

Again, in the same volume, lecture 12th, p. 424. re-
 ferring to what went before, he tells us, “ The know-
 “ ledge of the foregoing particulars is absolutely neces-
 “ sary for setting an undershot wheel to work; but the
 “ advantage to be reaped from it would be still guess-
 “ work, and we should be still at a loss to find out the
 “ utmost it can perform, if we had not an ingenious
 “ proposition of that excellent mechanic M. PARENT, of
 “ the Royal Academy of Sciences, who has given us a
 “ *maximum* in this case, by shewing, that an undershot
 “ wheel can do the most work, when its velocity is equal
 “ to the third part of the velocity of the water that
 “ drives it, &c. because then two-thirds of the water is
 “ employed in driving the wheel with a force propor-
 “ tionable to the square of its velocity. If we mul-
 “ tiply the surface of the adjutage or opening by the
 “ height of the water, we shall have the column of wa-
 “ ter that moves the wheel. The wheel thus moved will
 “ sustain on the opposite side only four-ninths of that
 “ weight,

“ weight, which will keep it in equilibrio; but what it
 “ can move with the velocity it goes with, will be but
 “ one-third of that weight of equilibrium; that is, $\frac{4}{27}$ ths
 “ of the weight of the first column, &c.—This is the
 “ utmost that can be expected.”

The same conclusion is likewise adopted by MACLAU-
 RIN, in art. 907. p. 728. of his *Fluxions*, where, giving
 the fluxionary deduction of M. PARENT’S proposition,
 he says, “ that if A represents the weight which would
 “ balance the force of the stream, when its velocity is
 “ a ; and u represents the velocity of the part of the en-
 “ gine, which it strikes when the motion of the machine
 “ is uniform, &c.—the machine will have the greatest
 “ effect when u is equal to $\frac{a}{3}$; that is, if the weight that
 “ is raised by the engine be less than the weight which
 “ would balance the power, in the proportion of 4 to 9,
 “ and the *momentum* of the weight is $\frac{4aa}{27}$.”

Finding that these conclusions were far from the
 truth; and seeing, from many other circumstances, that
 the practical theory of making water and wind-mills was
 but very imperfectly delivered by any author I had then
 an opportunity of consulting^(a); in the year 1751 I began
 a course

(a) BELIDOR, *Architectures Hydraulique*, greatly prefers the application of
 water to an undershot mill, instead of an overshot; and attempts to demonstrate,
 that water applied undershot will do fix times more execution than the same
 applied overshot. See vol I. p. 286. While DESAGULIERS, endeavouring to
 invalidate what had been advanced by BELIDOR, and greatly preferring an over-
 shot

a course of experiments upon this subject. These experiments, with the conclusions drawn from them, have already been communicated to this Society, who printed them in vol. LI. of their Transactions for the year 1759, and for this communication I had the honour of receiving the annual medal of Sir GODFREY COPLEY, from the hands of our very worthy President the late Earl of MACCLESFIELD. Those experiments and conclusions stand uncontroverted, so far as I know, to this day; and having since that time been concerned in directing the construction of a great number of mills, which were all executed upon the principles deduced from them, I have by that means had many opportunities of comparing the effects actually produced with the effects which might be expected from the calculation; and the agreement, I have always found between these two, appears to me fully to establish the truth of the principles upon

shot to an undershot, says, Annotat. on lecture 12. vol. II. p. 532. that from his own experience, “a well-made overshot mill ground as much corn in the same time with ten times less water;” so that betwixt BELIDOR and DESAGULIERS here is a difference of no less than 60 to 1.

Again, BELIDOR, vol. II. p. 72. says, that the center of gravity of each sail of a wind-mill should travel in its own circle with one-third of the velocity of the wind; so that, taking the distance of this center of gravity from the center of motion at 20 feet, as he states it p. 38. art. 849. the circumference will be exceeding 126 feet English measure: a wind, therefore, to make the mill go twenty turns *per* minute, which they frequently do with a sixth wind and all their cloth spread, would require the wind to move above eighty miles an hour; a velocity perhaps hardly equalled in the greatest storms we experience in this climate.

which

which they were constructed, when applied to great works, as well as upon a smaller scale in models.

Respecting the explanatory deduction of DESAGULIERS in the first example abovementioned, which, indeed, I have found to be the commonly received doctrine among theoretical mechanics, it is shewn, Phil. Transf. vol. LI. p. 120, 121, and 123. part I. maxim 4. that, where the velocity of water is double, the adjutage or aperture of the sluice remaining the same, the effect is eight times; that is, not as the square but as the cube of the velocity; and the same is investigated concerning the power of the wind arising from difference of velocity, p. 156. being part 3. maxim 4.

The conclusion in the second example abovementioned, adopted both by DESAGULIERS and MACLAURIN, is not less wide of the truth than the foregoing; for if that conclusion were true, only $\frac{4}{27}$ ths of the water expended could be raised back again to the height of the reservoir from which it had descended, exclusively of all kinds of friction, &c. which would make the actual quantity raised back again still less; that is, less than one-seventh of the whole; whereas it appears, from table I. p. 115. of the said volume, that in some of the experiments there related, even upon the small scale on which they were tried, the work done was equivalent to the raising back again about one quarter of the water expended; and in large works the effect is still greater, approaching towards half, which seems to be the limit for the undershot mills, as the whole would be the limit for
the

the overshot mills, if it were possible to set aside all friction, resistance from the air, &c. see p. 130.

The velocity also of the wheel, which, according to M. PARENT's determination, adopted by DESAGULIERS and MACLAURIN, ought to be no more than one-third of that of the water, varies at the *maximum* in the above-mentioned experiments of table 1. between one third and one half; but in all the cases there related, in which the most work is performed in proportion to the water expended, and which approach the nearest to the circumstances of great works, when properly executed, the *maximum* lies much nearer to one half than one third; one half seeming to be the true *maximum*, if nothing were lost by the resistance of the air, the scattering of the water carried up by the wheel, and thrown off by the centrifugal force, &c. all which tend to diminish the effect more, at what would be the *maximum* if these did not take place, than they do when the motion is a little slower.

Finding these matters, as well as others, to come out in the experiments, so very different from the opinions and calculations of authors of the first reputation, who, reasoning according to the Newtonian definition, must have been led into these errors from a want of attending to the proper collateral circumstances; I thought it very material, especially for the practical artist, that he should make use of a kind of reasoning in which he should not be so liable to mistakes; in order, therefore, to make this matter perfectly clear to myself, and possibly so to others,

I resolved

I resolved to try a set of experiments from whence it might be inferred, what proportion or quantity of mechanical power is expended in giving the same body different degrees of velocity. This scheme was put in execution in the year 1759, and the experiments were then shewn to several friends, particularly my very worthy and ingenious friend Mr. WILLIAM RUSSELL.

In my experimental inquiry concerning the powers of water and wind before referred to, I have, p. 105. part 1. defined what I meant by power, as applied to practical mechanics, that is, what I now call mechanical power; which, in terms synonymous to those there used, may be said to be measured by multiplying the weight of the body into the perpendicular height from which it can descend; thus the same weight, descending from a double height, is capable of producing a double mechanical effect, and is therefore a double mechanical power. A double weight descending from the same height is also a double power, because it likewise is capable of producing a double effect; and a given body, descending through a given perpendicular height, is the same power as a double body descending through half that perpendicular; for, by the intervention of proper levers, they will counter-balance one another, conformably to the known laws of mechanics, which have never been controverted. It must, however, be always understood, that the descending body, when acting as a measure of power, is supposed to descend slowly, like the weight of a clock or a jack; for, if quickly descending,

it is sensibly compounded with another law, *viz.* the law of acceleration by gravity.

DESCRIPTION OF THE MACHINE.

AB is the base of the machine placed upon a table.

AC is a pillar or standard.

CD is an arm, upon the extremity of which is fixed a plate *fg*, which is here seen edge-ways, through which is a small hole for receiving a small steel pivot *e*, fixed in the top of the upright axis *eB*; the lower end of this axis finishes in a conical steel point, which rests upon a small cup of hard steel polished at B.

HI is a cylinder of white fir, which passes through a perforation in the axis, and therein fixes; and, upon the two arms formed thereby, are capable of sliding

KL two cylindric weights of lead of equal size, which are capable of being fixed upon any part of the cylindric arms, from the axis to their extremities, by means of two thin wedges of wood. The two weights, therefore, being at equal distances from the center, and the axis perpendicular, the whole will be balanced upon the point at B, and moveable thereupon by an impelling power, with very little friction.

Upon the upper part of the axis are formed MN, two cylindrical barrels, whereof M is double the diameter of N; they have a little pin stuck into one side of each at *o*, *p*.

Q is a piece capable of sliding higher or lower, as occasion requires; and carries

R a light pulley of about three inches diameter, hung upon a steel axis, and moveable upon two small pivots. The plane of the pulley, however, is not directed to the middle of the upright axis, but a little on one side, so as to point (at a mean) between the surface of the bigger barrel and the less.

s is a light scale for receiving weights, and hangs by a small twine, cord, or line, that passes the pulley, and terminates either upon the bigger barrel or the less, as may be required; the sliding-piece Q being moved higher or lower for each, that the line, in passing from the pulley to the barrel, may be nearly horizontal. The end of the line, that is furthest from the scale, is terminated by a small loop, which hangs on upon the pin o, or the pin p, according as the bigger or the lesser barrel is to be used.

Now, having wound up a certain number of turns of the line upon the barrel, and having placed a weight in the scale s, it is obvious, that it will cause the axis to turn round, and give motion to its arms, and to the weights of lead placed thereon, which are the heavy bodies to be put in motion by the impulse of the weight in the scale; and when the line is wound off to the pin, the loop flips off, and the scale then falling down, the weight will cease to accelerate the motion of the heavy bodies, and leave them revolving, equably forward, with the velocity they have acquired, except so far as it must be gradually lessened by the friction of the machine and resistance of the air, which being small, the bodies will revolve sometime before their velocity is apparently diminished.

MEASURES OF SOME PARTS OF THE MACHINE.

	inches.
Diameter of the cylinders of lead, or the heavy bodies,	2,57
Length of ditto,	1,56
Diameter of the hole therein,	,72

Weight of each cylinder 3 lbs. Avoirdupois.

Greater distance of the middle of each body from the center of the axis,	8,25
The smaller distance of ditto,	3,92
10 turns of the smaller barrel raises the scale, 5 ditto of the bigger ditto,	25,25

When the bodies are at the smaller distance above specified from the axis of rotation, they are then in effect at half the greater distance from that axis; for, since the axis itself, and the cylindric arms of wood, keep an unvaried distance from the center of rotation, the bodies themselves must be moved nearer than half their former distance, in order that, compounded with the unvariable parts, they may be virtually at the half distance. In order to find this half distance nearly, I put in an arm of the same wood, that only went through the axis, without extending in the opposite direction; one of the bodies being put upon the end of this arm, at the distance of 8,25 inches, the whole machine was inclined till the body and arm became a kind of pendulum, and vibrated

at the rate of 92 times *per* minute; and as a pendulum of the half length vibrates quicker in the proportion of $\sqrt{1}$ to $\sqrt{2}$; that is, in the proportion of 92 to 130 nearly; therefore, keeping the same inclination of the machine, the weight was moved upon the arm till it made 130 vibrations *per* minute; which was found to be, when it was at 3,92 inches distance from the center as above stated, which is about $\frac{2}{10}$ ths of an inch nearer than the half distance. The double arm was then put in, and marked accordingly, and the bodies being mounted thereon, the whole was adjusted ready for use; and with it were tried the following experiments, each of which was repeated so many times as to be fully satisfactory.

TABLE OF EXPERIMENTS.

No	Ounces Avoirdupois in the Scale.	Barrel used, M the bigger, N the smaller.	The Arms, W the whole, H the Half- length.	Number of Turns of the Line wound on the Barrel.	Time of the Descent of the Weight in the Scale.	Time in mak- ing 20 Revolu- tions with equa- ble Motion.
1	8	M	W	5	14 $\frac{1}{4}$	29
2	8	N	W	10	28 $\frac{1}{4}$	29 $\frac{1}{4}$
3	8	N	W	2 $\frac{1}{2}$	14 $\frac{1}{4}$	58 $\frac{1}{2}$
4	32	M	W	5	7	14
5	32	N	W	10	14	14 $\frac{3}{4}$
6	32	N	W	2 $\frac{1}{2}$	7	28 $\frac{3}{4}$
7	8	M	H	5	7	14 $\frac{3}{4}$
8	8	N	H	10	14	15
9	8	N	H	2 $\frac{1}{2}$	7	30 $\frac{1}{4}$
1	2	3	4	5	6	7

The 58 $\frac{1}{2}$ in number 3, column 7, was determined in fact from 29 $\frac{1}{4}$, being the time of making 10 equable revolutions, after the weight was dropped off, in order to prevent the sensible retardation that might take place, and affect the observation, if continued for 20 revolutions made so slowly.

FURTHER DEFINITIONS.

I have already defined what I mean by mechanic power; but, before I proceed further, it will be necessary also to define the following terms:

Impulse or Impulsion,	} By all which, I understand the uniform endeavour that one body exerts upon another, in order to make it move; and that, whether it produces or generates motion by this endeavour or not; and the quantity of this impelling power may be measured either by its being a weight of itself, or by being counter-balanced by a weight. It may also act either immediately upon the body to be moved, so that if motion is the consequence, they move with the same velocity; and that, either by a simple contact, or by being drawn as by a cord, or pushed as by a staff: or it may act by the intervention of a lever or other mechanic instrument, in which the velocity of the body to be moved may be very different from the velocity of the impelling power or mover; but in comparing them, the impelling powers must be reduced according to the proportional velocities of the mover and moved; or, in levers of different lengths, they may be compared by a standard length of lever, which is the method taken in the subsequent reasoning upon the preceding experiments.
Impulsive Force or Power,	
Impelling Force or Power,	

An impelling power, therefore, consisting of a double weight, or requiring a double weight to counter-balance it, when acting with equal levers, is a double impelling power, or an impelling power of double the intensity.

OBSER-

OBSERVATIONS AND DEDUCTIONS FROM THE PRECEDING
EXPERIMENTS.

1st, By the first experiment it appears, that the mechanic power employed, consisting of 8 ounces in the scale, deliberately descending (by 5 turns of the bigger barrel) through a perpendicular space $25\frac{1}{4}$ inches, will represent the quantity of mechanic power which causes the two heavy bodies, from a state of rest, to acquire a velocity, such as to carry them equably through 20 circumferences of their circle of revolution in the space of 29"; and that the time in which the mechanic power produced this effect was $14\frac{1}{4}$ ", as appears by column 6th. And this mechanic power we shall express by the number 202, the product of the number of ounces in the scale multiplied by the inches in its perpendicular descent, for $8 \times 25\frac{1}{4} = 202$.

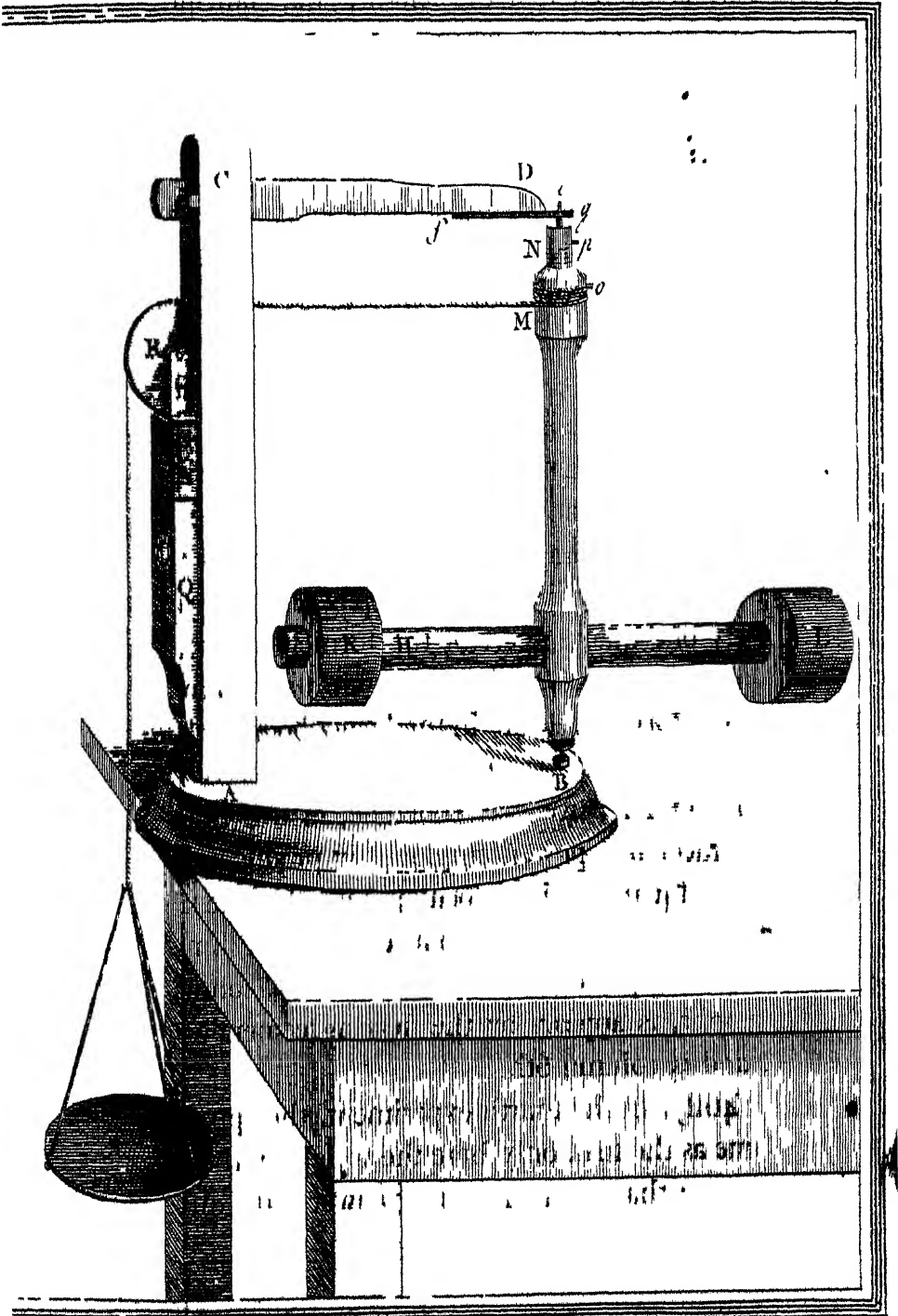
2d, By the second experiment, as 10 turns of the smaller barrel are equal to the same perpendicular height as 5 turns of the bigger, it follows, that the same mechanic power, *viz.* 202, acting upon the same heavy bodies to accelerate them, produces the very same effect in generating motion in the bodies as it did before, *viz.* 20 revolutions in $29\frac{1}{4}$ ", the small difference of $\frac{1}{4}$ of a second being no more than may reasonably be attributed to the unavoidable errors arising from friction of the machine, want of perfect accuracy in its measures, resistance of the air, and imperfections in the observations themselves, which must not only be allowed for in this, but

the rest; but as the impelling power is acting here upon a lever of but half the length, and, consequently, but half the intensity, when referred to the bodies to be moved, it takes just double the time to generate the same velocity therein.

DEDUCTION. It appears from hence, that the same mechanic power is capable of producing the same velocity in a given body, whether it is applied so as to produce it in a greater or a lesser time; but that the time taken to produce a given velocity, by an uniformly continued action, is in a simple inverse proportion of the intensity of the impulsive power.

3dly, The third experiment being made with $2\frac{1}{2}$ turns of the lesser barrel, the same weight in the scale of 8 ounces descending only one quarter part of the former perpendicular, the mechanic power employed will be only one quarter part of the former, *viz.* $50\frac{1}{2}$; but as one quarter part of the mechanic power produces half of the former velocity in the heavy bodies; that is, they make 20 revolutions in $58\frac{1}{2}$; that is, nearly 10 revolutions in 29"; we may conclude, in this instance, that the mechanic power, employed in producing motion, is as the square of the velocity produced in the same body; and that the velocity produced is as the time that an impelling power, of the same intensity, continues to act upon it, as appears by the near agreement of numbers 2 and 3, column 6th.

4thly, In the fourth experiment, the apparatus is the same as the first, only here the weight in the scale is 32 ounces; that is, the impelling power is the quadruple of
the



the first, and hereby a double velocity is given to the bodies; for they make 20 revolutions in $14''$, which is a small matter less than half the time taken up in making 20 revolutions in the first experiment. It also appears, that the velocity acquired is simply as the impelling power compounded with the time of its action; for a quadruple impulsions acting for $7''$ instead of $14''$ generates a double velocity, while the mechanic power employed to generate it is quadruple, for $32 \times 25\frac{1}{4} = 808$. And here the mechanic power employed being four times greater than the first, it holds here also, that the mechanic power, to be necessarily employed, is as the square of the velocity to be generated; that is, in the same proportion as turned out in the third experiment, where the mechanic power employed was only a quarter part of the first.

5thly, The fifth and sixth experiments were made with a mechanic power four times greater than those employed in numbers 2 and 3 respectively; and since the same deductions result from hence as from numbers 2 and 3, they are additional confirmations of the conclusions drawn from them and from the last article.

6thly, In the seventh experiment, the disposition of the apparatus is the same as number 1, only here the bodies are placed upon the arms at the half-length; from whence it appears, that the same mechanic power still produces the same velocity in the same bodies; for though 20 revolutions were performed in $14\frac{3}{4}''$ (see column 7)

which is nearly half the time that 20 revolutions were performed in the first experiment; yet, since the circles in which the bodies revolved in the seventh are only of half the circumference as those of number 1, it is obvious, that the absolute velocity acquired by the moving bodies in the two cases is equal. But, by column 6th, the time in which it was generated is only half; yet, notwithstanding, this will coincide with the former conclusions, if the intensity of the impelling power is compounded therewith; for, though the barrel was the same with the same number of turns as in number 1, and therefore the lever the same, by which the impelling power acted, yet, as the bodies, upon which this lever was to act, were placed upon a lever of only half the length from the center, the impelling power, acting by the first lever, would act upon the second with double the intensity, according to the known laws of mechanics; that is, it would require a double weight opposing the bodies, to prevent their moving, in order to balance it. An impulsive power, therefore, of double the intensity, acting for half the time, produces the same effect in generating motion, as an impulsive power, of half the intensity, acting for the whole time.

7thly, The eighth and ninth experiments afford the same deductions and confirmations relative to the seventh experiment, that the fifth and sixth do respecting the fourth, and that the second and third do respecting the first; and from the near agreement of the whole, when
the

the necessary allowances before mentioned are made, together with some small inequality arising from the mechanical power lost by the difference of the motion given by gravity to the weight in the scale: I say, from these agreements, under the very different mechanical powers applied, which were varied in the proportion of 1 to 16, we may safely conclude, that this is the universal law of nature, respecting the capacities of bodies in motion to produce mechanical effects, and the quantity of mechanic power necessary to be employed to produce or generate different velocities (the bodies being supposed equal in their quantity of matter); that the mechanic powers to be expended are as the squares of the velocities to be generated, and *vice versa*; and that the simple velocities generated are as the impelling power compounded with, or multiplied by, the time of its action, and *vice versa*.

We shall, perhaps, form a still clearer conception of the relation between velocities produced, and the quantities of mechanic power required to produce them; together with the collateral circumstances attending, by which these propositions, seemingly two, are reconciled and united, by stating the following popular elucidation, which, indeed, was the original idea that occurred to me on considering this subject; to put which to an experimental proof gave birth to the foregoing apparatus and experiments.

Suppose then a large iron ball of 10 feet diameter, turned truly spherical, and set upon an extended plane of the same metal, and truly level. Now, if a man begins to

Q q q 2

push

push at it, he will find it very resisting to motion at first; but, by continuing the impulse, he will gradually get it into motion, and having nothing to resist it but the air, he will, by continuing his efforts, at length get it to roll almost as fast as he can run. Suppose now, in the first minute he gets it rolled through a space of one yard; by this motion, proceeding from rest (similar to what happens to falling bodies) it would continue to roll forward at the rate of two yards *per* minute, without further help; but supposing him to continue his endeavours, at the end of another minute he will have given it a velocity capable of carrying it through a space of two yards more, in addition to the former, that is, at the rate of four yards *per* minute; and at the end of the third minute, he has again added an equal increase of velocity, and made it proceed at the rate of six yards *per* minute; and so on, increasing its velocity at the rate of two yards in every minute. The man, therefore, in the space of every minute exerts an equal impulse upon the ball, and generates an equal increase of movement correspondent to the definition of Sir ISAAC NEWTON. But let us see what happens besides: the man, in the first minute, has moved but one yard from where he set out; but he must in the second minute move two yards more, in order to keep up with the ball; and as he exerted an impulse upon it, so as at the end of second minute to have given it an additional velocity of the two yards, he must also in this time have gradually changed its velocity from the rate of two yards *per* minute to that of four, and the space, that he will of conse-

quence have actually been obliged to go through in the second minute, will be according to the mean of the extremes of velocity at the beginning and end thereof, that is, three yards in the second minute; so that being one yard from his original place at the beginning of the second minute, at the end of it he will have moved the sum of the journies of the first and second minute, that is, in the whole four yards from his original place. As he has now generated a velocity in the ball of four yards *per* minute, in the third minute he must travel four yards to keep up with the ball, and one more in generating the equal increment of velocity; so that in the third minute, he must travel five yards to keep up the same impelling power upon the ball that he did in the first minute in travelling one, so that these five yards in the third minute, added to the four yards that he had travelled in the two preceding minutes, sets him at the end of the third minute nine yards from whence he set out, having then given the ball a velocity capable of carrying it uniformly forward at the rate of six yards *per* minute, as before stated. We may now leave the further pursuit of these proportions, and see how the account stands. He generated a velocity of two yards *per* minute in the first minute, the square of which is four, when he had moved but one yard from his place; and he had generated a velocity of six yards *per* minute, the square of which is thirty-six, at the end of the third minute, when he had travelled nine yards from his place. Now, since the square of the velocity, generated at the end of the first minute, is to that

of the velocity generated at the end of the third minute, as 4 : 36, that is, as 1 : 9; and since the spaces, moved through by the man to communicate these velocities, are also as 1 : 9, it follows, that the spaces through which the man must travel, in order to generate these velocities respectively (preserving the impelling power perfectly equal), must be as the squares of the velocities that are communicated to the ball; for, if the man was to be brought back again to his original place by a mechanical power, equally exerted upon the man equally resisting, this would be the measure of what the man has done in order to give motion to the ball. It therefore directly follows, conformably to what has been deduced from the experiments, that the mechanic power that must of necessity be employed in giving different degrees of velocity to the same body, must be as the square of that velocity; and if the converse of this proposition did not hold, *viz.* that if a body in motion, in being stopped, would not produce a mechanical effect equal or proportional to the square of its velocity, or to the mechanical power employed in producing it, the effect would not correspond with its producing cause.

Thus the consequences of generating motion upon a level plane exactly correspond with the generating of motion by gravity; *viz.* that though in two seconds of time the equal impulsive power of gravity gives twice the velocity to a body that it does in one second, yet this collateral circumstance attends it, that at the end of the double time, in consequence of the velocity acquired in

the first half, the body has fallen from where it set forward through four times the perpendicular; and, therefore, though the velocity is only doubled, yet four times the mechanical power has been consumed in producing it, as four times the mechanical power must be expended in bringing up the fallen body to its first place.

This then appears to be the foundation, not only of the disputes that have arisen, but of the mistakes that have been made, in the application of the different definitions of quantity of motion; that while those, that have adhered to the definition of Sir ISAAC NEWTON, have complained of their adversaries, in not considering the time in which effects are produced, they themselves have not always taken into the account the space that the impelling power is obliged to travel through, in producing the different degrees of velocity. It seems, therefore, that, without taking in the collateral circumstances both of time and space, the terms, quantity of motion, *momentum*, and force of bodies in motion, are absolutely indefinite; and that they cannot be so easily, distinctly, and fundamentally compared, as by having recourse to the common measure, *viz.* mechanic power.

From the whole of what has been investigated, it therefore appears, that time, properly speaking, has nothing to do with the production of mechanical effects, otherwise than as, by equally flowing, it becomes a common measure; so that, whatever mechanical effect is found to be produced in a given time, the uniform continuance of the action of the same mechanical power will, in a double time,

time, produce two such effects, or twice that effect. A mechanical power, therefore, properly speaking, is measured by the whole of its mechanical effect produced, whether that effect is produced in a greater or a lesser time; thus, having treasured up 1000 tuns of water, which I can let out upon the over-shot wheel of a mill, and descending through a perpendicular of 20 feet, this power applied to proper mechanic instruments, will produce a certain effect, that is, it will grind a certain quantity of corn; and that, at a certain rate of expending it, it will grind this corn in an hour. But suppose the mill equally adapted to produce a proportionable effect, by the application of a greater impulsive power as with a less, then, if I let out the water twice as fast upon the wheel, it will grind the corn twice as fast, and both the water will be expended and the corn ground in half an hour. Here the same mechanical effect is produced; *viz.* the grinding a given quantity of corn, by the same mechanical power, *viz.* 1000 tuns of water descending through a given perpendicular of 20 feet, and yet this effect is in one case produced in half the time of the other. What time, therefore, has to do in the business is this: let the rate of doing the business, or producing the effect, be what it will, if this rate is uniform, when I have found by experiment what is done in a given time, then, proceeding at the same rate, twice the effect will be produced in twice the time, on supposition that I have a supply of mechanic power to go on with. Thus 1000 tuns of water, descending through 20 feet of perpendicular,

dicular, being, as has been shewn, a given mechanic power, let me expend it at what rate I will, if when this is expended, I must wait another hour before it be renewed, by the natural flow of a river, or otherwise, I can then only expend twelve such quantities of power in 24 hours; but if, while I am expending 1000 tuns in one hour, the stream renews me the same quantity, then I can expend 24 such quantities of power in 24 hours; that is, I can go on continually at that rate, and the product or effect will be in proportion to time, which is the common measure; but the quantity of mechanic power arising from the flow of the two rivers, compared by taking an equal portion of time, is double in the one to the other, though each has a mill, that, when going, will grind an equal quantity of corn in an hour.

XXVIII. *A new and general Method of finding simple and quickly-converging Series; by which the Proportion of the Diameter of a Circle to its Circumference may easily be computed to a great Number of Places of Figures.*
By Charles Hutton, Esq. F. R. S.

TO THE REV. DR. HORSLEY, SEC. R. S.

S I R,

Royal Mil. Acad.
Jan. 25, 1776.

R. May 1, 1776. **I**N a late examination of the methods of Mr. MACHIN and others, for computing the proportion of the diameter of a circle to its circumference, I discovered the method which is explained in the paper accompanying this letter. This method you, SIR, will perceive is very general, and discovers many series which are very fit for the abovementioned purpose. If you think it has sufficient merit to entitle it to the honour of being offered to the Royal Society, I have taken the liberty to inclose it to you, requesting the favour of you to present it accordingly, from, &c.

P. S. In the course of the last year, I attended some very interesting experiments on the effects of the force

force of fired gun-powder; as they seem to lead to many important conclusions, I believe I shall, in a short time, have the honour of submitting to your inspection an account of some of them, for the like purpose as the following paper.

THE excellency of this method is primarily owing to the simplicity of the series by which an arc is found from its tangent. For if t denote the tangent of an arc a , the radius being 1, then it is well known, that the arc a will be equal to the infinite series,

$$\frac{t}{1} - \frac{t^3}{3} + \frac{t^5}{5} - \frac{t^7}{7} + \frac{t^9}{9} - \frac{t^{11}}{11} + \&c.$$

where the form is as simple as can be desired. And it is evident, that nothing farther is required than to contrive matters so, as that the value of the quantity t in this series may be both a small and a very simple number. Small, that the series may be made to converge sufficiently fast; and simple, that the several powers of t may be raised by easy multiplications, or easy divisions.

Since the first discovery of the above series, many have used it, and that after different methods, for determining the length of the circumference to a great number of figures. Among these were Dr. HALLEY, Mr. ABRAHAM SHARP, Mr. MACHIN, and others, of our own country; and M. DE LAGNY, M. EULER, &c. abroad. Dr. HALLEY used the arc of 30° , or $\frac{1}{12}$ th of the circumference, the tangent of which being $=\sqrt{\frac{1}{3}}$, by substituting $\sqrt{\frac{1}{3}}$ for t in the above series, and multiplying by 6, the semi-

circumference is $= 6\sqrt{\frac{1}{3}} \times 1 - \frac{1}{3 \cdot 3} + \frac{1}{5 \cdot 3} - \frac{1}{7 \cdot 3} + \frac{1}{9 \cdot 3}$, &c. which series is, to be sure, very simple; but its rate of converging is not very great, on which account a great many terms must be used to compute the circumference to many places of figures. By this very series, however, the industrious Mr. SHARP computed the circumference to 72 places of figures; Mr. MACHIN extended it to 100; and M. DE LAGNEY, still by the same series, continued it to 128 places of figures. But although this series, from the 12th part of the circumference, does not converge very quickly, it is, perhaps, the best aliquot part of the circumference which can be used for this purpose; for when smaller arcs, which are exact aliquot parts, are used, their tangents, although smaller, are so much more complex, as to render them, on the whole, more operose in the application; this will easily appear, by inspecting some instances, that have been given by Mr. GARDINER, in his editions of SHERWIN'S Tables. One of these methods is from the arc of 18° , the tangent of which is $\sqrt{1 - 2\sqrt{\frac{1}{5}}}$; another is from the arc of $22\frac{1}{2}^\circ$, the tangent of which is $\sqrt{2} - 1$; and a third is from the arc of 15° , the tangent of which is $2 - \sqrt{3}$. All of which are evidently too complex to afford an easy application to the general series.

In order to a still farther improvement of the method by the above general series, Mr. MACHIN, by a very singular and excellent contrivance, has greatly reduced the
Labour

labour naturally attending it. His method is explained in Mr. MASERES's Appendix to his Differtation on the Use of the Negative Sign in Algebra; and I have given an analysis of it, or a conjecture concerning the manner in which it is probable Mr. MACHIN discovered it, in my Treatise on Mensuration; which, I believe, are the only two books in which that method has been explained, as I never had seen it explained by any, till I met with Mr. MASERES's book abovementioned on the Use of the Negative Sign. For though the series' discovered by that method were published by Mr. JONES, in his *Synopsis Palmariorum Matheseos*, which was printed in the year 1706, he has given them merely by themselves, without the least hint of the manner in which they were obtained. The result shews, that the proportion of the diameter to the circumference is equal to that of 1 to quadruple the sum of the two series',

$$\left\{ \begin{array}{l} \frac{4}{5} \times : 1 - \frac{1}{3 \cdot 5^2} + \frac{1}{5 \cdot 5^4} - \frac{1}{7 \cdot 5^6} + \frac{1}{9 \cdot 5^8}, \text{ \&c.} \\ \frac{1}{239} \times : 1 - \frac{1}{3 \cdot 239^2} + \frac{1}{5 \cdot 239^4} - \frac{1}{7 \cdot 239^6} + \frac{1}{9 \cdot 239^8}, \text{ \&c.} \end{array} \right.$$

The slower of which converges almost thrice as fast as Dr. HALLEY's raised from the tangent of 30°. The latter of these two series converges still a great deal quicker; but then the large incompofite number 239, by the reciprocals of the powers of which the series converges, occasions such long, tedious divisions, as to counter-balance its quickness of convergency; so that the former series is summed, with rather more ease than the latter, to the same number.

ber of places of figures. Mr. JONES, in his *Synopsis*, mentions other series' besides this, which he had received from Mr. MACHIN for the same purpose, and drawn from the same principle. But we may conclude this to be the best of them all, as he did not publish any other besides it.

M. EULER too, in his *Introductio in Analysin Infinitorum*, by a contrivance something like Mr. MACHIN's, discovers, that $\frac{1}{2}$ and $\frac{1}{3}$ are the tangents of two arcs, the sum of which is just 45° ; and that, therefore, the diameter is to the circumference as 1 to quadruple the sum of the two series',

$$\begin{cases} \frac{1}{2} \times : 1 - \frac{1}{3 \cdot 4} + \frac{1}{5 \cdot 4^2} - \frac{1}{7 \cdot 4^3} + \frac{1}{9 \cdot 4^4}, \&c. \\ \frac{1}{3} \times : 1 - \frac{1}{3 \cdot 9} + \frac{1}{5 \cdot 9^2} - \frac{1}{7 \cdot 9^3} + \frac{1}{9 \cdot 9^4}, \&c. \end{cases}$$

Both which series' converge much faster than Dr. HALLEY's, and are yet at the same time made to converge by the powers of numbers producing only short divisions; that is, divisions performed in one line, or without writing down any thing besides the quotients.

I come now to explain my own method, which, indeed, bears some little resemblance to the methods of MACHIN and EULER; but then it is more general, and discovers, as particular cases of it, both the series' of those gentlemen and many others, some of which are fitter for this purpose than theirs are.

This method then consists, in finding out such small arcs, as have for tangents some small and simple vulgar fractions (the radius being denoted by 1), and such also, that

some multiple of those arcs shall differ from an arc of 45° , the tangent of which is equal to the radius, by other small arcs, which also shall have tangents denoted by other such small and simple vulgar fractions. For it is evident, that if such a small arc can be found, some multiple of which has such a proposed difference from an arc of 45° , then the lengths of these two small arcs will be easily computed from the general series, because of the smallness and simplicity of their tangents; after which, if the proper multiple of the first arc be increased or diminished by the other arc, the result will be the length of an arc of 45° , or $\frac{1}{8}$ th of the circumference. And the manner in which I discover such arcs is thus:

Let τ , t , denote any two tangents, of which τ is the greater, and t the less; then it is known, that the tangent of the difference of the corresponding arcs is equal to $\frac{\tau-t}{1+\tau t}$.

Hence, if t , the tangent of the smaller arc, be successively denoted by each of the simple fractions $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, &c. the general expression for the tangent of the difference between the arcs will become respectively

$\frac{2\tau-1}{2+\tau}$, $\frac{3\tau-1}{3+\tau}$, $\frac{4\tau-1}{4+\tau}$, $\frac{5\tau-1}{5+\tau}$, &c.; so that if τ be expounded,

by any given number, then these expressions will give the tangent of the difference of the arcs in known numbers, according to the values of t , severally assumed respectively. And if, in the first place, τ be equal to 1, the tangent of 45° , the foregoing expressions will give the tangent of an arc, which is equal to the difference between that of 45° and the first arc; or that, of which the

tangent is one of the numbers $\frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \frac{1}{5}, \&c.$ Then if the tangent of this difference, just now found, be taken for τ , the same expressions will give the tangent of an arc, which is equal to the difference between the arc of 45° and the double of the first arc. Again, if for τ we take the tangent of this last found difference, then the foregoing expressions will give the tangent of an arc, equal to the difference between that of 45° and the triple of the first arc. And again taking this last found tangent for τ , the same theorem will produce the tangent of an arc equal to the difference between that of 45° and the quadruple of the first arc; and so on, always taking for τ the tangent last found, the same expressions will give the tangent of the difference between the arc of 45° and the next greater multiple of the first arc; or that, of which the tangent was at first assumed equal to one of the small numbers $\frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \frac{1}{5}, \&c.$ This operation, being continued till some of the expressions give such a fit, small, and simple fraction as is required, is then at an end, for we have then found two such small tangents as were required, *viz.* the tangent last found, and the tangent first assumed.

Here follow the several operations adapted to the several values of t . The letters $a, b, c, d, \&c.$ denote the several successive tangents.

1. Take $t = \frac{1}{2}$, then the theorem $\frac{2\tau-1}{2+\tau}$ gives

$$a = \frac{1}{3}$$

$$b = \frac{-1}{7}$$

Therefore the arc of 45° , or $\frac{1}{8}$ th of the circumference, is
either

either equal to the sum of the two arcs of which $\frac{1}{2}$ and $\frac{1}{3}$ are the tangents, or to the difference between the arc of which the tangent is $\frac{1}{7}$, and the double of the arc of which the tangent is $\frac{1}{2}$; that is, putting Λ = the arc of 45° .

$$\Lambda = \begin{cases} +\frac{1}{2} \times : 1 - \frac{1}{3 \cdot 4} + \frac{1}{5 \cdot 4^2} - \frac{1}{7 \cdot 4^3} + \frac{1}{9 \cdot 4^4} - \&c. \\ +\frac{1}{3} \times : 1 - \frac{1}{3 \cdot 9} + \frac{1}{5 \cdot 9^2} - \frac{1}{7 \cdot 9^3} + \frac{1}{9 \cdot 9^4} - \&c. \end{cases}$$

$$\text{Or, } \Lambda = \begin{cases} +1 - \frac{1}{3 \cdot 4} + \frac{1}{5 \cdot 4^2} - \frac{1}{7 \cdot 4^3} + \frac{1}{9 \cdot 4^4} - \&c. \\ -\frac{1}{7} \times : 1 - \frac{1}{3 \cdot 49} + \frac{1}{5 \cdot 49^2} - \frac{1}{7 \cdot 49^3} + \frac{1}{9 \cdot 49^4} - \&c. \end{cases}$$

And the former of these values of Λ is the same with that before mentioned as given by M. EULER; but the latter is much better, as the powers of $\frac{1}{49}$ converge much faster than those of $\frac{1}{9}$.

COROL. From double the former of these values of Λ subtracting the latter, the remainder is,

$$\Lambda = \begin{cases} +\frac{2}{3} \times : 1 - \frac{1}{3 \cdot 9} + \frac{1}{5 \cdot 9^2} - \frac{1}{7 \cdot 9^3} + \frac{1}{9 \cdot 9^4} - \&c. \\ +\frac{1}{7} \times : 1 - \frac{1}{3 \cdot 49} + \frac{1}{5 \cdot 49^2} - \frac{1}{7 \cdot 49^3} + \frac{1}{9 \cdot 49^4} - \&c. \end{cases}$$

which is a much better theorem than either of the former.

2. If z be taken $= \frac{1}{3}$, then the expression $\frac{3^x - 1}{3 + x}$ gives,

$$b = \frac{1}{7}$$

Here the value of $x = \frac{1}{2}$ gives the same expression for the value of Λ as the first in the foregoing case, and the value

of $b = \frac{1}{7}$ gives the value of A the very same as in the corollary to the case above.

3. Taking $t = \frac{1}{4}$, the expression $\frac{4^x - 1}{4 + 1}$ gives

$$a = \frac{3}{5}$$

$$b = \frac{7}{23}$$

$$c = \frac{5}{99}$$

$$d = -\frac{79}{401}$$

Here it is evident, that the value of $c = \frac{5}{99}$ is the fittest number afforded by this case; and from it it appears, that the arc of 45° is equal to the sum of the arc of which the tangent is $\frac{5}{99}$, and the triple of the arc of which the tangent is $\frac{1}{4}$.

$$\text{Or that } A = \begin{cases} +\frac{3}{4} \times : 1 - \frac{1}{3 \cdot 16} + \frac{1}{5 \cdot 16^2} - \frac{1}{7 \cdot 16^3} + \frac{1}{9 \cdot 16^4} - \&c. \\ +\frac{5}{99} \times : 1 - \frac{5^2}{3 \cdot 99^2} + \frac{5^4}{5 \cdot 99^4} - \frac{5^6}{7 \cdot 99^6} + \frac{5^8}{9 \cdot 99^8} - \&c. \end{cases}$$

Which is the best theorem that we have yet found, because that the number 99 resolves into the two easy factors 9 and 11.

4. Let now t be taken $= \frac{1}{5}$, and the expression $\frac{5^x - 1}{5 + 1}$ will give

2

$$b = \frac{7}{17}$$

$$c = \frac{9}{46}$$

$$d = \frac{-1}{239}$$

Where

Where it is evident, that the last number, or the value of d , is the fittest of those produced in this case, and from which it appears, that the arc of 45° is equal to the difference between the arc of which the tangent is $\frac{1}{239}$, and quadruple the arc of which the tangent is $\frac{1}{5}$. Or that

$$A = \left\{ \begin{aligned} & + \frac{4}{5} \times : 1 - \frac{1}{3 \cdot 5^2} + \frac{1}{5 \cdot 5^4} - \frac{1}{7 \cdot 5^6} + \frac{1}{9 \cdot 5^8} - \&c. \\ & - \frac{1}{239} \times : 1 - \frac{1}{3 \cdot 239^2} + \frac{1}{5 \cdot 239^4} - \frac{1}{7 \cdot 239^6} + \frac{1}{9 \cdot 239^8} - \&c. \end{aligned} \right.$$

Which is the very theorem that was invented by Mr. MACHIN, as we have before mentioned.

5. Take now $t = \frac{1}{6}$, and the expression $\frac{6\pi-1}{6+\pi}$ will give

$$a = \frac{5}{7}$$

$$b = \frac{23}{47}$$

$$c = \frac{91}{305}$$

$$d = \frac{241}{1921}$$

$$e = \frac{-475}{11767}$$

Of which numbers none, it is evident, are fit for our purpose.

6. Again, take $t = \frac{1}{7}$, and the expression $\frac{7t-1}{7+t}$ will give

$$a = \frac{3}{4}$$

$$- \frac{17}{31}$$

$$11$$

$$d = \frac{49}{205}$$

$$e = \frac{69}{742}$$

$$f = \frac{-259}{5263}$$

Neither are any of these fit numbers for our purpose.

7. In like manner take $t = \frac{1}{8}$, so shall $\frac{8t-1}{8+t}$ give

$$a = \frac{7}{9}$$

$$b = \frac{47}{79}$$

$$c = \frac{297}{679}$$

$$d = \frac{1697}{5729}$$

$$e = \frac{7847}{47529}$$

$$f = \frac{14047}{388079}$$

8. And

. And if t be taken $= \frac{1}{9}$, the expreffion $\frac{9t-1}{9+t}$ will give

$$a = \frac{4}{5}$$

$$\frac{31}{49}$$

$$c = \frac{115}{236}$$

$$d = \frac{799}{2239}$$

$$e = \frac{2467}{10475}$$

$$f = \frac{11809}{96751}$$

$$g = \frac{4765}{441284}$$

. Also, if we take $t = \frac{1}{10}$, the expreffion $\frac{10t-1}{10+t}$ will give .

$$a = \frac{9}{11}$$

$$b = \frac{79}{119}$$

$$c = \frac{671}{1269}$$

$$d = \frac{5441}{13361}$$

$$e = \frac{41049}{139051}$$

$$f = \frac{271439}{1431559}$$

$$g = \frac{1282831}{14587029}$$

10. Farther, if we take $t = \frac{1}{11}$, the expreffion $\frac{11t-1}{11+t}$ will give

$$a = \frac{1}{6}$$

$$b = \frac{49}{71}$$

$$c = \frac{234}{415}$$

$$d = \frac{2159}{4799}$$

$$e = \frac{9475}{27474}$$

$$f = \frac{76751}{311689}$$

$$g = \frac{266286}{1752665}$$

$$h = \frac{1176481}{19545601}$$

11. Laftly, if we take $t = \frac{1}{12}$, the expreffion $\frac{12t-1}{12+t}$ gives

$$a = \frac{11}{13}$$

$$b = \frac{113}{167}$$

$$c = \frac{41}{73}$$

$$d = \frac{419}{917}$$

$$e = \frac{4111}{11423}$$

$$f = \frac{37909}{141187}$$

$$g = \frac{313721}{1732153}$$

$$h = \frac{2032499}{21099557}$$

$$i = \frac{3290431}{255227183}$$

Here

Here it is evident, that none of these latter cases afford any numbers that are fit for this purpose. And to try any other fractions less than $\frac{1}{12}$ for the value of t , does not seem likely to answer any good purpose, especially, as the divisors, after 12, become too large to be managed in the easy way of short division in one line.

By the foregoing means it appears, that we have discovered five different forms of the value of A , or $\frac{1}{4}$ th of the semi-circumference, all of which are very proper for readily computing its length; *viz.* three forms in the first case and its corollary, one in the third case, and one in the fourth case. Of these, the first and last are the same as those invented by EULER and MACHIN respectively, and the other three are quite new, as far as I know.

But another remarkable excellency attending the first three of the before mentioned series is, that they are capable of being changed into others which not only converge still faster, but in which the converging quantity shall be $\frac{1}{10}$, or some multiple or sub-multiple of it, and so the powers of it raised with the utmost ease. The series, or theorems, here meant are these three:

$$\begin{aligned}
 \text{1st, } A = & \left\{ \begin{aligned} & +\frac{1}{2} \times \frac{1}{3 \cdot 4} + \frac{1}{5 \cdot 4^2} - \frac{1}{7 \cdot 4^3} + \frac{1}{9 \cdot 4^4}, \&c. \\ & +\frac{1}{3} \times : 1 - \frac{1}{3 \cdot 9} + \frac{1}{5 \cdot 9^2} - \frac{1}{7 \cdot 9^3} + \frac{1}{9 \cdot 9^4}, \&c. \\ & + 1 - \frac{1}{3 \cdot 4} + \frac{1}{5 \cdot 4^2} - \frac{1}{7 \cdot 4^3} + \frac{1}{9 \cdot 4^4}, \&c. \end{aligned} \right. \\
 \text{2dly, } A = & \left\{ \begin{aligned} & -\frac{1}{7} \times : 1 - \frac{1}{3 \cdot 49} + \frac{1}{5 \cdot 49^2} - \frac{1}{7 \cdot 49^3} + \frac{1}{9 \cdot 49^4}, \&c. \end{aligned} \right. \\
 & \text{3dly,}
 \end{aligned}$$

$$3\text{dly, } A = \begin{cases} +\frac{2}{3} \times : 1 - \frac{1}{3 \cdot 9} + \frac{1}{5 \cdot 9} - \frac{1}{7 \cdot 9^3} + \frac{1}{9 \cdot 9^5}, \&c. \\ +\frac{1}{7} \times : 1 - \frac{1}{3 \cdot 49} + \frac{1}{5 \cdot 49^2} - \frac{1}{7 \cdot 19^3} + \frac{1}{9 \cdot 19}, \&c. \end{cases}$$

Now if each of these be transformed, by means of the differential series in cor. 3. p. 64. of the late Mr. THOMAS SIMPSON'S Mathematical Differtations, they will become of these very commodious forms, viz.

$$\begin{aligned} 1\text{st, } A &= \begin{cases} +\frac{4}{10} \times : 1 + \frac{4}{3 \cdot 10} + \frac{8\alpha}{5 \cdot 10} + \frac{12\epsilon}{7 \cdot 10} + \frac{16\gamma}{9 \cdot 10}, \&c. \\ +\frac{3}{10} \times : 1 + \frac{2}{3 \cdot 10} + \frac{4\alpha}{5 \cdot 10} + \frac{6\epsilon}{7 \cdot 10} + \frac{8\gamma}{9 \cdot 10}, \&c. \end{cases} \\ 2\text{dly, } A &= \begin{cases} +\frac{4}{5} \times : 1 + \frac{4}{3 \cdot 10} + \frac{8\alpha}{5 \cdot 10} + \frac{12\epsilon}{7 \cdot 10} + \frac{16\gamma}{9 \cdot 10}, \&c. \\ -\frac{7}{50} \times : 1 + \frac{4}{3 \cdot 100} + \frac{8\alpha}{5 \cdot 100} + \frac{12\epsilon}{7 \cdot 100} + \frac{16\gamma}{9 \cdot 100}, \&c. \end{cases} \\ 3\text{dly, } A &= \begin{cases} +\frac{6}{10} \times : 1 + \frac{2}{3 \cdot 10} + \frac{4\alpha}{5 \cdot 10} + \frac{6\epsilon}{7 \cdot 10} + \frac{8\gamma}{9 \cdot 10}, \&c. \\ +\frac{7}{50} \times : 1 + \frac{2}{3 \cdot 50} + \frac{4\alpha}{5 \cdot 50} + \frac{6\epsilon}{7 \cdot 50} + \frac{8\gamma}{9 \cdot 50}, \&c. \end{cases} \end{aligned}$$

Where $\alpha, \epsilon, \gamma, \&c.$ denote always the preceding terms in each series.

Now it is evident, that all these latter series' are much easier than the former ones, to which they respectively correspond; for, because of the powers of 10 here concerned, we have little more to do than to divide by the series of odd numbers 1, 3, 5, 7, 9, &c.

Of all these three forms the second is the fittest for computing the required proportion; because that, of the two series' of which it consists, the several terms of the one

are found from the like terms of the other, by dividing these latter by 10 and its several successive powers 100, 1000, &c.; that is, the terms of the one consist of the same figures as the terms of the other, only removed a certain number of places farther towards the right, in the decuple scale of numbers; and the number of places, by which they must be removed, is the same as the distance of each term from the first term of the series, *viz.* in the second term the figures must be moved one place lower, in the third term two, in the fourth term three, &c. so that the latter series will consist of but about half the number of the terms of the former. Thus, then, this method may be said to effect the business by one series only, in which there is little more to do, than to divide by the several numbers 1, 3, 5, 7, &c.; for as to the multiplications by the numbers in the numerators of the terms, after they become large, they are easily performed by barely multiplying by the number two, and subtracting one number from another: for since every numerator is less by two than the double of its denominator, if d denote any denominator (exclusive always of the powers of 10) then the co-efficient of that term is $\frac{2d-2}{d}$, or $2 - \frac{2}{d}$, by which the preceding term is to be multiplied; to do which, therefore, multiply it by two, that is double it, and divide that double by the divisor d , and subtract the quotient from the said double.

By a pretty exact estimate, which I have made, of the proportion of the trouble or time in computing the circumference by this middle form of the value of A, and by Mr. MACHIN's theorem, I have found, that the computation by his method requires about $\frac{1}{8}$ th or $\frac{1}{10}$ th more time than by mine. And its advantage over any of the series' invented by EULER or others, is still much more considerable.

XXIX. *An Account of a very extraordinary Effect of Lightning on a Bullock, at Swanborow, in the Parish of Iford near Lewes, in Suffex. In sundry Letters, from Mr. James Lambert, Landscape-Painter at Lewes; and One from William Green, Esquire, at Lewes, to William Henly, F. R. S.*

LETTER I.

FROM MR. LAMBERT.

Sept. 13, 1774.

R. May 1,
1776. I SHALL now inform you of a very extraordinary and singular effect of lightning on a bullock in this neighbourhood, which happened about a fortnight since. The bullock is pyed, white and red. The lightning, as supposed, stripped off all the white hair from his back, but left the red hair without the least injury. I have been to see the bullock, and have made a drawing of it, which I will send you as soon as I can get more particulars from Mr. Rogers, the proprietor; for, when I saw him, I omitted to ask him, if the hair was all off (as it now appears) when it was first seen the next morning; and whether any hair was found in the field; and if it appeared to be singed or not? The

bullock does not seem to have been hurt; his skin looks fair and well. Mr. ROGERS informs me, that he has had two other bullocks struck in the same manner; one last summer, that was all white, was stripped of his hair like this, though not all over his back, but chiefly on his shoulders; the other, two years since, was pyed, and affected much like the present. He thinks, it cannot be the effect of any disease, because the beasts were all in good health before and after this accident happened. He is more inclined to think it was the effect of lightning, because when he has had cattle disordered, so as to make their hair come off, he has never observed white hair to come off more than red, &c.; but that it has, if party-coloured, fallen off promiscuously, and generally in patches; and also by slow degrees, and never suddenly, as in the case of these bullocks. I shall be glad to know your thoughts on this matter, whether it is a new circumstance to you or not; and if you think it much worth attending to. I am, &c.

L E T T E R II.

FROM WILLIAM GREEN, ESQ.

S I R,

Sept. 28, 1774.

THE inclosed account of the effect of lightning seems to me very extraordinary; perhaps, such instances may be known to you; however, to be certain whether they

they are so or not, I have troubled you with a description of this case; if it should prove to be no novelty, you will only have the trouble of reading it.

In the evening of Sunday the twenty-eighth of August, at this place, there was an appearance of a thunder storm, but we heard no report. A gentleman, who was riding near the marshes not far distant from this town, saw two strong flashes of lightning, seemingly running along the ground of the marsh, at about nine of the clock in the evening. On Monday morning, when the servants of Mr. ROGERS, a farmer at Swanborough, in the parish of Iford, went into the marsh, to fetch the oxen to their work, they found one of them, a four-year-old steer, standing up, to appearance much burnt, and so weak as to be scarce able to walk. This was mentioned to me about a week after the accident happened; and by the description of it, it seemed to have been struck with lightning in a very uncommon manner. The ox is of a red and white colour; the white in large marks, beginning at the rump-bone, and running, in various directions, along both the sides; the belly is all white, and the whole head and horns are white likewise. The lightning, with which it must have been undoubtedly struck, fell on the rump-bone, which is white, and distributed itself along the sides, in such a manner, as to take off all the white hair from the white marks as low as the bottom of the ribs, but so as to leave a list of white hair, about half an inch broad, all round where it joined to the red; and not a single hair of the red, that I can perceive, is touched.

touched. The whole belly is unhurt, but the end of the sheath of the *penis* has the hair taken off; it is also taken off from the *deulap*; the horns and the curled hair on the forehead are uninjured, but it is taken off the sides of the face, from the flat part of the jaw-bones, and it is taken off from the front of the face in stripes. There are a few white marks on the side and neck, which are surrounded with red, and the hair is taken off from them, leaving half an inch of white adjoining to the red. I looked attentively at the feet and legs, and could not discover any hair taken from them (they were very dirty) except from the joint a little above one of the hoofs, where it was partly off. I have sent a sketch of one of the sides of the ox, which will serve to illustrate what I have said, and is better than any description. I have coloured, with faint red, those parts which were stripped of the hair. The farmer anointed the ox with oil for a fortnight; the animal purged very much at first, and is greatly reduced in flesh. I saw it about a fortnight ago, and it was then recovering. I am, &c.

L E T T E R III.

FROM MR. LAMBERT.

Nov. 15, 1774.

I AM sorry for the delay in sending you this drawing; but, as you know the cause, I need not say any thing further than that I have the happiness to acquaint you,
that

that I am now quite recovered from my illness. I have not had an opportunity of seeing Mr. GREEN, therefore cannot tell how his account may agree with mine; but I have endeavoured, as much as possible, to get an exact state of the matter, having carefully inspected the bullock twice, accompanied by my nephew and a gentleman at my house. The creature being, as I observed to you before, remarkably gentle, we could examine every part of him very minutely. You will see by the drawing, that the white hair was all stripped off from his back and down the sides, as low as the greatest diameter of his body, also from the top of the nose, the upper part of both cheeks, and over the eyes, leaving the skin quite bare; but below those places, under the belly, gullet, the under parts of the cheeks, the legs, and ring of white in the tail, together with an edge of white at the parting of the red and white hair, all remained without the least injury. We were the more particular in examining the legs, on account of your mentioning that Mr. GREEN had observed traces of the stroke down them to the ground, in which, I think, he must be mistaken; for, both the times when we saw the bullock, his legs were quite clean down to the hoofs, and the hair seemed to be in a perfect state. If the legs had been at all affected, I think, it could not have escaped our notice in two examinations; and there being no marks of lightning on them, inclines me to think, that the bullock was lying down at the time, and if so, you will readily account for the under parts not being touched. The lightning being conducted by the white
hair,

hair, from the top of the back down the sides, came to the ground, at the place where the white hair is left entire; but there is one remarkable circumstance, *viz.* though all the white hair on the upper parts was taken away, as beforementioned, yet the tuft of white hair on the forehead never received any hurt at all. I have conversed with several farmers, &c. in hopes of getting some information relative to those matters, but can meet with nothing perfectly satisfactory. The best account I have been able to obtain is from my neighbour Mr. TOOTH, a farrier and bullock-leach. He tells me, that this circumstance is not new to him; that he has seen a great many pyed bullocks struck by lightning in the same manner as this, both in his father's time (his father being of the same trade) and since; and that the texture of the skin under the white hair was always destroyed, though looking fair at first; and, after a while, it became sore, throwing out putrid matter in pustules, like the small pox with us, which in time falls off, when the hair grows again as before; and that the bullocks receive no further injury. In this state I found Mr. ROGERS's bullock, the second time I saw it, which was about a month after the first visit; some of the scabs were then dropping off, and the hair was coming on afresh. I asked Mr. TOOTH, whether he could recollect among those bullocks which he, or his father, had seen struck dead by lightning, any that were white or pyed? But in this he could not satisfy me; if he could, I think, it would have thrown some light on the subject. I

remember perfectly well, that all the cattle that I have seen, which were killed by lightning, were either black, brown, or red, without any white at all in them. I must observe to you, that this bullock is both marked and affected by the stroke exactly alike on both sides.

I am, &c.

HAVING been favoured with these letters, by gentlemen of the strictest veracity, and likewise particularly curious in their enquiries, I have not the least reason to entertain a doubt of the facts they communicate; and as they may, perhaps, be productive of some important discoveries, respecting the different colours of bodies as conductors of electricity, I imagined, that it would not be improper to lay them before the members of the Royal Society.

To the preceding paper I would beg leave to add the following queries:

1st, Are not the dark-coloured hairs stronger in their texture than the white or light-coloured ones^(a)?

2dly, If the dark-coloured hairs are the strongest, may not this be owing to their being more deeply rooted, and partaking more largely of that nutritive matter which produces and supports hair? And does not the change of dark-coloured hair to grey, in persons advanced in years, seem to favour this supposition?

(a) This is a fact so well known to house-painters, that they do not admit a dark hair into their brushes, as they would occasion a disagreeable roughness in their work. J. COVENTRY.

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3dly,

3dly, If the above suppositions are allowed, may not any internal injury to the skin (as a violent electric explosion passing through it) prove more fatal to the white or light hair than to the black, red, or darker colours?

Should the above queries be all acknowledged, by those gentlemen who have considered the subject, to be truths; yet, I believe, they will scarcely be thought sufficient to account for the whole effect of lightning which has appeared in this case, and particularly for the edge of white hair adjoining to the red, which remained unhurt by it; but as they may, perhaps, in some measure have contributed towards this phenomenon, I have barely mentioned them as suppositions, and suppositions only, which have occurred to me.

A SUBSEQUENT LETTER FROM MR. LAMBERT.

DEAR SIR,

Dec. 6, 1774.

I HAVE, according to your desire, enquired of Mr. TOOTH, whether he ever saw a stroke of lightning actually fall upon a pyed bullock, so as to destroy the white hair, and shew the evident marks of burning, leaving the red hair uninjured? He says, he never did; nor can he recollect any one that has. But he gives me an account of a pyed horse of his being killed, four or five years since, in a stable adjoining to his house, by a stroke of lightning which happened in the night; and being very great, Mr. TOOTH thinking it struck his house, immediately

immediately got up and went to the stable, when he saw his horse was struck, and almost dead to appearance, though it kept on its legs near half an hour before it expired. The horse was pyed white on the shoulder and most part of the head; that is, all the forehead and nose, where the greatest force of the stroke came. The hair was not burnt nor discoloured, only so loosened at the root, that it came off at the least touch. And this is the case, Mr. TOOTH observes, with all he has seen or heard of, *viz.* the hair is never burnt, but the skin always affected, as I described it in my former letters. In the above horse, Mr. TOOTH says, all the blood in the veins, under the white parts of the head, was quite stagnated, though he could perceive it to flow as usual in other parts of the body, under the brown hair; and the skin, together with one side of the tongue, was parched and dried up to a greater degree than any he had ever seen before. The horse stood in a stall close to the door of the stable, which was boarded on that side, and through them, he thinks, the lightning struck him. I am, &c.

*Extract of another Letter from Mr. LAMBERT, dated Oct.
10, 1775, with a Drawing N^o 3.*

I HERE send you another instance of the effect of lightning on a bullock of Mr. ALSE's, at Glynd, which happened on the 20th of last month; it is similar to the other I sent you in every respect, except that I think the stroke on this must have been greater, as the scarf-skin

seems to be peeling off with the hair all over the rump, like the piece I have herewith sent, which came off from the hip. I think too, that this is more curious, as all the red spots, even those small ones on the side, remain firm and smooth, without the least injury. You will observe also, that, as in the former instance, after the lightning had passed the greatest diameter of the body, the white hair is left entire, particularly under the belly, on the legs, &c. Mr. ALSE, having never seen nor heard of this wonderful phenomenon, could not conceive what was the matter with the bullock, till he sent for Mr. TOOTH, who immediately told him the cause. I am, &c.

HAVING mentioned the foregoing particulars to my learned and ingenious friend Dr. A. FOTHERGILL, at Northampton, he has favoured me with some conjectures, which I shall take the liberty of annexing to this paper; *viz.* "The recent fact you mention, of the effects of lightning on the white hair of a bullock, is extremely curious, but seems difficult of solution. Whether it can be explained from the difference of texture between red hair and white, is doubtful; or whether there is not something peculiar in colours, as being conductors or non-conductors of electricity, may deserve enquiry. The phlogiston, or inflammable principle, is thought to be the foundation of colour in bodies, and to abound in proportion to the intensity of the colour. But phlogiston and the electric fluid are
 " probably

“ probably the same, or at least modifications of the same
“ principle; therefore, red bodies are perhaps replete
“ with electric matter, while white bodies may be desti-
“ tute of it^(b). A body saturated with it cannot receive
“ more, and may escape, while a neighbouring body, not
“ calculated to receive it, may, on its admission, be de-
“ stroyed^(c). Or there may exist a chemical affinity be-
“ tween electricity and the different rays of light, which,
“ in attracting some, and repelling others, may be the
“ foundation of many curious phenomena. But, while
“ we admire the effects; the habitudes and *modus ope-*
“ *randi* of these subtile fluids may, perhaps, for ever
“ elude the cognizance of our senses.”

(b) Many substances must certainly be excepted from this rule. W. HENLY.

(c) This effect of lightning generally happens to such bodies which, in some measure, resist its entrance, &c. merely on account of their being imperfect conductors. W. HENLY.

XXX. *Of the Light produced by Inflammation.*

By George Fordyce, M. D. F. R. S.

R. May 9, 1776. **W**HEN a body is heated to a certain degree, it becomes luminous, and is said to be ignited. One of the means of producing heat is inflammation; and this, as is well known, is sufficient for ignition. But, I apprehend, that, besides the light produced by ignition, there is also light produced by the inflammation itself. For the investigation of this principle, it will be necessary to consider ignition in the first place.

Substances, heated to between 6 and 700° of FAHRENHEIT's thermometer, begin to be luminous in the dark. If they be colourless, the light which is first observed is red; as the heat is increased, there is a mixture of yellow rays; and, lastly, a due proportion of all the coloured rays to form a pure white, which has been commonly called, by chemists, a melting heat. The intensity of this light depends much upon the density of the heated body; for, while metals, heated to this degree, throw out a strong light, the vapour at the end of the flame of a blow-pipe, properly applied to a lamp, is not visibly luminous, though the heat be so great as immediately to give a white heat to glass. The colour of this light is
affected

affected by the colour of the ignited matter. While *zinc* is calcining, the pure white *calx* throws off a light, which vies with that of the Sun in brightness and purity; the green *calx* of copper gives to the flame of a fire, in which it is calcining, a beautiful green; and tallow burning in a candle, being converted into empyreumatic oil, as it passes off from the wick, the yellow colour of this oil gives a yellowness to the flame, which very much alters the colours of objects seen by candle-light from what they appear to be in the day.

The light produced by the decomposition of bodies in inflammation is totally independent of the heat, and its colour is blue; for substances which burn, without producing 600° of heat of FAHRENHEIT's thermometer, give light during their inflammation. Thus, *phosphorus* of urine exposed to the air burns and is decomposed, producing light with very little heat; and that this is a true inflammation and decomposition appears from this experiment. Take a receiver of white glass, capable of holding six or eight gallons; put into it a drachm of *phosphorus* of urine, finely powdered, and half an ounce of water; cork the mouth of the receiver, and tie it over with a bladder, so as to exclude the external air; incline the receiver to all sides gently, and afterwards set it to rest; the powder will adhere to the sides, and the water will drain from it. As soon as the water is sufficiently drained off, the particles of the *phosphorus* will become luminous, and emit a thick smoke: this will continue for some days; but at last no more light or vapour will appear.

appear. Open the receiver, and you will find, that the air will have contracted, as it does from the inflammation of a candle in VAN HELMONT's experiment; that is, about a twentieth part. It is become unfit for inflammation; for if a lighted candle be immersed in it, it will be extinguished as well as the *phosphorus*, and an animal will be suffocated by it. The air then has suffered the same change, as that air which has served for the inflammation of other bodies; and the *phosphorus* is partly decomposed, the water in the receiver being impregnated with its acid, and the air saturated with its phlogiston. Blow fresh air into the receiver, and the light and smoke will immediately re-appear. In like manner, it is known, that sulphur will burn and give light, without heat sufficient for ignition. Take a piece of iron heated nearly red hot, and throw a little gun-powder upon it. If the heat be of a proper degree, the sulphur will burn off with a blue flame, without heat sufficient for ignition; for, if such heat had been produced, the gun-powder would certainly have taken fire, which it does not. It is the inflammation and decomposition of the sulphur, and not its evaporation, which produces the light; for, if we sublime sulphur in close vessels, made of the most transparent glass, no light will be visible, except at the very beginning, when a small portion of it burns till the air in the vessel be saturated, and rendered unfit for inflammation.

That the light, which is produced by the decomposition in inflammation, is blue, in whatever degree of heat
the

the inflammation takes place, appears from observing the bottom and lower part of the flame of a candle, where the inflammation is; the light produced is blue. Or take a candle which has burnt for some time, extinguish it by applying tallow to the wick, and let it stand to cool; afterwards set it on fire by the flame of another candle: at first, no more vapour will arise than can be acted upon by the air at once; inflammation, therefore, will go on in the whole small flame, and it will be blue. It may be necessary to observe here, that, when a candle burns, the following process happens. The tallow boils in the wick, and is converted into empyreumatic oil, rising from it in the form of vapour. As it rises from every part of the wick, the volume is increased till it comes to the top, and gives to the lower part of the flame the form of a *frustum* of an inverted cone. The air is applied to the outer surface of the column of vapour, and, there decomposing the empyreumatic oil, produces heat and blue light; the *stratum* of vapour, within the outer burning surface, is heated white-hot; the heat diminishes towards the center, which, if the flame be large, is scarcely red-hot; as the column rises, decomposition taking place constantly on its surface, it necessarily diminishes, and the upper part of the flame is conical. That the tallow boils in the wick can be seen; that it is converted into empyreumatic oil is proved by drawing the vapour, rising in the middle of the flame, where it does not burn, into a glass tube; the empyreumatic oil condenses. This also shews, that the flame does not burn in the middle. That the heat is

produced on the outer surface appears, if we take a small rod of glass, and put the end of it in the blue flame on the surface; it will be heated white-hot and melt. Immerse the rod into the flame, so that the point shall be in the center, it will melt and bend, where it is in the blue flame on the surface; whereas, if the flame be large, the point which is in the center will hardly be heated red-hot. That the empyreumatic oil is decomposed is proved by burning a candle with a very small wick in distilling vessels, no condensation of empyreumatic oil takes place. We may conclude, therefore, that light is produced by the decomposition, as well as by the ignition, in inflammation.

I will not take up the time of this learned Society in applying these principles to the explanation of various appearances in burning bodies; or ground upon them any practical rules for producing strong or faint, white or coloured light, these being sufficiently obvious. I have chosen to illustrate this subject by experiments that may be the most easily tried; but, lest the manner of powdering *phosphorus* should not be known, I will give the process. Take *phosphorus* of urine two drachms; put it into a four-ounce phial; pour upon it three ounces of water; heat it gently, by immersion in warm water, till the *phosphorus* melts; shut the phial with a cork; take it out of the water, and shake it briskly till it be cold; the *phosphorus* will be found in powder.

XXXI. *Experiments on ignited Bodies.*

By John Roebuck, M. D. F. R. S.

*Extract of a Letter from Dr. ROEBUCK to Dr.
BROCKLESBY.*

R. Feb. 15,
1776. **M**R. BUFFON asserts, that he found a ball of iron, which weighed 49 lbs. 9 oz. when cold, to weigh, when heated to a white heat, 49 lbs. 11 oz. which is an augmentation of weight of $19\frac{1}{5}$ grains to the pound.

This extraordinary fact, circumstantially narrated by the very eminent and ingenious M. BUFFON, being contrary to the opinions of those philosophers who have most enlarged our natural Knowledge by their candid and cautious inquiry into the qualities of bodies, made me very solicitous to make similar experiments.

Some time ago, when I was at Birmingham, I had very luckily an opportunity, by the aid of two accurate balances of my friend Mr. BOLTON'S; one of which would, without straining the beam, weigh a pound and turn with one-tenth of a grain; and the other weigh half an ounce, and turn with the hundredth part of a grain.

X x x 2

I heated

I heated a piece of iron, of nearly one pound weight, to a white heat, or what the smiths call a welding heat, and found, by the most accurate experiments which I could make, and which I again and again repeated, that the iron, when left several hours in the balance to cool, weighed nearly one grain less when cold than when hot; and that a piece of iron, of about five pennyweights, which was tried by the smaller and more accurate balance, weighed, as appeared by an index which moved opposite to a quadrant, somewhat more when cold than when hot. I tried the same experiment on copper; but, to my great surprize, I found a piece of copper, of nearly one pound weight, four grains lighter after it had been left some hours to cool in the balance than when it was put in. I repeated the experiment, and found the event the same; but suspecting this might possibly arise from the copper casting scales, I placed a sheet of white paper under the balance, and collected as many scales as made up nearly the deficiency of the weight.

TO SIR JOHN PRINGLE, BART. P. R. S.

S I R,

London,
May 9, 1776.

R₁ May 9, 1776. I BEG leave to inform you of the following experiments, which were made to ascertain the variation of the weight of bodies when hot and cold.

April

April 29, 1776, I heated a cylinder of wrought iron, which weighed fifty-five pounds, to a white heat, and exactly balanced the same, when hot, in the presence of the hon. HENRY CAVENDISH, NATH. PICCOT, MATTHEW RAPER, ANDREW CROSBY, EDW. DELAVAL, HAMILTON, DAVID HARTLEY, WILLIAM RUSSEL, Esquires; Doctors HUNTER, BROCKLESBY, MORTON, WILLIAM FORDYCE, GEORGE FORDYCE, RUSSEL, WATSON junior, MUSGRAVE; Mess. JOHN HUNTER, BEN. WILSON, JAMES RUSSEL, RAMSDEN, WHITEHOUSE, MAGELLAN. After the cylinder had been two hours cooling in the scale, I weighed it again, and found that it had increased in weight three pennyweights and a few grains. Five hours after cooling, Mr. MAGELLAN weighed it, and found it had increased in weight three pennyweights seventeen grains. Six hours after cooling, when the cylinder was blood-warm, I weighed it again, in the presence of Dr. HUNTER, Dr. BROCKLESBY, Mr. GRAY, and Mr. NEISBIT, and found it to have increased in weight four pennyweights. The day following, about twenty-two hours after cooling in the presence of MATTHEW RAPER and ANDREW CROSBY, esquires, I again weighed it, and found that it had increased in weight six pennyweights seventeen grains. Mr. ABRAM WHITEHOUSE, who was very solicitous to have the above experiment made accurately, procured from Mr. SAMUEL READ a very exact beam, which readily turned, to the conviction of all the above gentlemen, with less than a pennyweight, though loaded with the above iron cylinder; but MATTHEW RAPER, ANDREW CROSBY, Esquires, and myself

self examined the beam leifurely and accurately, and found it turned very diftinctly with four grains, though loaded as above. In order to difcover the caufe of this increafe of weight of the cylinder when cold, I heated two ounces eight pennyweights of the fcales or *calx* of wrought iron, and found the fame to increafe in weight five grains when cold.

I heated two pieces of pure filver which weighed two pounds, ten ounces, five pennyweights; and when the filver was cold it increafed in weight five grains, though it produces no *calx* from being heated red-hot.

The above experiments are conformable to thofe which I formerly made with fome very exact fcales at Birmingham; an account of which I tranfmitted to Dr. BROCKLESBY.

I have the honour to be, &c.

XXXII. *Experiments and Observations on a new Apparatus, called, A Machine for exhibiting perpetual Electricity. In a Letter to the Rev. Dr. Horsley, Sec. R. S. from Mr. William Henly, F. R. S.*

REVEREND SIR,

March 4, 1776.

R. May 16,
1776. **M**Y ingenious friend Mr. GEORGE ADAMS, philosophical instrument-maker to his majesty, lately put into my hands a little apparatus, which he called a machine for exhibiting perpetual electricity, and informed me, that it was the invention of some foreign gentleman^(a). This machine consisted of a circular plate of glass, about eight inches in diameter, covered on one side with a coating of bees-wax and rosin, about the sixteenth part of an inch thick. This coat of wax, &c. being strongly excited with a dry warm flannel, he placed upon it a circular board, of the same dimensions, coated with tin-foil, and furnished with a glass handle screwed to, and standing upright upon it. These bodies having remained in contact some seconds, the board was raised up by the glass handle; when, applying the knuckle to the tin-foil coating, a snap was heard, a spark seen, and a small sensation felt. On replacing the board, and permitting it to remain some seconds, as before, having touched the tin-foil with a finger, on removing it again, and applying the knuckle, as at first, the same

(a) I have since learned from Mr. NAIRNE, that M. VOLTA, of Coma, near Milan, was the inventor of it.

phenomena

phenomena were produced; and might, Mr. ADAMS observed, be repeated for a long time, without any renewal of the excitation of the wax, any farther than the replacing the board might be said to excite it. It immediately occurred to me, that, as this plate of wax, &c. was made by excitation, a strong negative electric, the phenomena produced by it could only be the reverse of those I had formerly made with an excited plate of glass, and published in the Phil. Transf. vol. LXIV. part II. p. 407.; viz. where mine were positive, these were negative; and where mine were negative, these were positive. But, to determine this matter, I made the following experiments. First, I insulated Mr. CANTON's electrometer, and having electrified the balls positively, I presented toward them the excited wax, as soon as it had been separated from the coated board; and perceived, as I expected, that the balls were attracted by the wax; but, if the balls were electrified negatively, they were as plainly repelled by it. The board produced just the contrary effect. Secondly, I held my Leyden *vacuum*, or analysis of the Leyden bottle, described Phil. Transf. vol. LXIV. part II. p. 400. by the coated bulb, and touched the brass ball on the neck of it with the coated board, the moment it had been separated from the excited wax, &c. and instantly perceived a variety of beautiful streams dart from the point of the wire in the bottle, and spread themselves in different directions through the bulb. On repeating the experiment, and presenting the coated part of the bottle toward the board, a small spark of light appeared upon the point of the inclosed wire; a plain indication

indication that the point had received electricity, and, as before observed, that the coated board, being separated from the wax, &c. was strongly electrified *plus*; and consequently, the coating of wax, &c. on the plate of glass. *minus*. These phenomena, being so often produced, without a fresh excitation of the wax, though they are astonishing to strangers, will not be so surprizing to electricians, who have considered Mr. GREY's experiment with a cone of sulphur, contained in a glass vessel, which, as often as they were separated, shewed signs of electricity in all states of the weather. See Dr. PRIESTLEY's History of Electricity, 2d edit. p. 39. I have shewn at large, in a former paper, that merely heating either glass or amber will not make them electrical; but the friction of glass against glass, or sealing-wax against sealing-wax, previously warmed, I find, will excite either of these substances; and my ingenious and learned friend THOMAS RONAYNE, Esq. informs me, that he had long since made the same remark on sealing-wax. But, pressing a finger in the gentlest manner on the amber, after heating, will excite it. Indeed, a fine piece, which I frequently carry in my pocket, I always perceive to be electrical, without any other friction than what it receives from the pocket. Sealing-wax, Mr. RONAYNE tells me, he always found to be affected in the same manner; and negative electrics, *per se*, being once thoroughly excited, are observed to retain their electrical quality very long, as they do not so soon attract the moisture in the atmosphere as glass. Glass, however, will retain its electricity many hours, as I have had frequent occasion to remark. My late friend

Mr. CANTON informed me, that, having excited a rod of glass very strongly, he set it at some distance from the fire in his parlour, and found that it was electrical, after standing in that situation, in dry air, twenty-four hours. How much longer it would have retained its electricity, had he let it remain there, he knew not. How long a large and neatly-prepared Leyden bottle will retain its charge, so as to be sensibly electrical, I have never experienced; but Dr. PRIESTLEY observes, History of Electricity, p. 516, that he has more than once received such shocks as he should not like to receive again from the *residuum* of his battery, even two days after the discharge, and when papers, books, his hat, and many other things, had lain upon the wires the greatest part of the time. Even the *residuum* of a *residuum*, he says, he has known to remain in his battery many days^(a). One thing, however, is very remarkable in Mr. ADAMS's apparatus, *viz.* supposing the negative electric to have parted with its electricity to the rubber; why, when the coated board or plate of metal is set upon it, and that plate is touched by a finger, the equilibrium is not thus presently restored? But, perhaps, when the electric matter, naturally inherent in bodies, is once thoroughly excited and put in

(a) My friend the reverend Mr. HEMMING, hath been so obliging as, at my request, to make a variety of experiments, with a view to determine this matter, and shewed me a small bottle, which attracted a thread of silk at one-sixteenth of an inch distance, May 23, though the bottle had been charged and stood in a cupboard in his study from March 14, *viz.* 70 days. The cylinder to his electrical machine will also separate the balls of Mr. CANTON's electrometer a fortnight after using, though a variety of methods have been repeatedly used to destroy that power in the interval.

action, it is not so soon as might be suspected reduced again to a quiescent state, especially in bodies so peculiarly adapted to affect each other as these appear to be. Mr. LANE has favoured me with a very curious experiment, which he made as long since as the month of June 1764, and then shewed to many of his friends, which seems fully to confirm this opinion. I have, therefore, requested his leave to insert it, as follows. Having procured two large pieces of thick plate glass A and B, with plain surfaces, and fitted them so as to coincide with each other, he coated a part (about eighteen square inches) of A, on one side, with tin-foil, and an equal part of B he coated in the same manner, so as to answer exactly to A, leaving a margin of glass, an inch and a half broad, in the narrowest part; but, at one of the ends of each plate (which end was reduced in breadth), not less than five inches of the glass were reserved uncoated, for the purpose of handling them. The uncoated sides of these glasses being laid together, they were charged by the machine as one plate; when the plate A, which touched the prime conductor, was found, on separating them, to be positive on both sides; and B, which was touched by a finger during the operation, was negative on both sides. Then, laying them in contact, as at first, and making the discharge as with the Leyden bottle, the plates were still found to cohere, and after separation were observed to remain strongly electrical; but with an electricity directly contrary to that they shewed before the discharge, A being now negative, and B positive on both side. But, what is particularly to my purpose, if the

Y y y 2 coating

coating on A and B (after laying them again together as at first) were touched, at the same time, by a thumb and finger of the hand, or any conductor communicating with the earth, the plates would then, on being separated (the experiment being made in a dark room) emit a strong flash of light; and this phenomenon Mr. LANE has frequently produced twelve or fourteen times successively, touching the coating of the plates each time before the separation, without renewing the charge in the glass by the machine; but if he omitted to touch the coatings as above mentioned, no light was visible on the separation of the plates (*b*). Should those gentlemen, if any such remain, who are of opinion, that in electrical experiments two fluids, the vitreous and the resinous, are concerned, proceed to make experiments of this kind, they may, perhaps, from some phenomena, be induced to draw conclusions which they may think not unfavourable to their own hypothesis.

My experiments with the excited plate of glass, published in the Philosophical Transactions, as before mentioned, may serve, however, as a key to explain both Mr.

(*b*) Crown-glass, that is, the glass commonly used for sash-windows, though so much thinner, succeeds in this experiment as well as the plate-glass; but what is very remarkable, the Dutch plates, when treated in the same manner, have each a positive and a negative surface, and the electricity of both surfaces, of both plates, is exchanged for the contrary electricity in the discharge. If a clean, dry, uncoated plate of looking-glass be placed between the coated looking-glass plates, or between the plates of crown-glass, it appears, after charging, to be negatively electrified on both sides; but if it be placed between the Dutch plates, it acquires, like them, a positive electricity on one surface, and a negative electricity on the other. Further particulars, with a description of some new electrical apparatus, constructed on account of these phenomena, will be given at another opportunity.

GREY's experiments, and those made with Mr. ADAMS's little apparatus now under consideration. Having procured a plate of glass, ten inches long and eight inches broad, coated in the manner of Mr. ADAMS's, I was inclined to pursue these inquiries somewhat farther. Accordingly, I placed, upon a strong supporter of glass, a circular board, with a smooth and flat surface; and upon this board I laid a circular brass plate, of nearly the same size; and lastly, I placed upon the brass plate Mr. CANTON's electrometer. Then, having excited the plate of wax with dry, warm flannel, experiment 3, I set it upon the insulated apparatus. The balls presently opened, and, on examination, appeared to be electrified negatively; but, on removing the wax, they closed, and opened again much wider, and were then found to be electrified positively. In this experiment, the quantity of electricity, naturally inherent in the balls, strings, &c. had been drawn up into the apparatus by the attractive power of the excited plate of wax, and they were thus left in a negative state; but, on removing the plate of wax, the balls closed again, in consequence of the return of the electricity, which would be increased if the plate of brass had been touched by a finger, &c. and the balls then became very powerfully electrified *plus*. By applying, in the same manner, the excited uncoated plate of glass, or the excited uncoated side of the same plate, the reverse of these phenomena took place, as I have before described them and referred to in the beginning of this paper.

Experiment 4. I insulated two of Mr. CANTON's electrometers, A and B, and having raised them in such a manner.

manner as to let the balls hang about the eighth part of an inch higher than the plate of brass on which the excited plate of wax was laid, I electrified the balls of A positively, and the balls of B negatively, so as to diverge about an inch; I then brought the insulated apparatus as near as I could to the balls, without affecting either, (the brass plate might then be at nearly an inch and an half distance from the nearest ball, both of A and B); then, suddenly removing the excited wax, the balls of B instantly flew to the brass plate, and those of A were, at the same instant, repelled to as great a distance from it. The apparatus having remained in this situation some seconds, on withdrawing the stand with the brass plate, &c. the balls of B closed, having received by this process the quantity of electricity they had before been deprived of; but the balls of A still remained separate, as wide as ever.

Experiment 5. Having replaced the excited wax, &c. upon the brass plate, I again electrified the balls of A and B, as in the former experiment, *viz.* those of A positively, and those of B negatively. I then took a small phial, properly prepared for the Leyden experiment, containing only about three square inches of coated surface; then, presenting the knob, on the wire of the phial, to the plate of brass, I removed the wax, &c. and instantly saw a strong spark between the brass plate and the knob of the phial: when, presenting that knob towards the balls of A, they were considerably repelled; but on presenting it toward those of B, they were as much attracted. I have made several other experiments with this apparatus; but, as they all agree with those
above

above mentioned, I think it unnecessary to recite them. I have likewise omitted to give a drawing; as to electricians, I apprehend, this paper will be intelligible without one; and to those who have not considered the subject, I imagine, it would be of very little, if indeed of any use whatever. The same difficulty which occurred to Dr. FRANKLIN, in his analysis of the Leyden bottle, may be said to occur also in this apparatus, *viz.* it is hard to say how, or where this electricity is deposited, there is so much of it; and it is so easily put in action, that I am still further confirmed in an opinion that I have long entertained, *viz.* that the slightest friction between bodies of every kind, in every situation, may disturb the electric matter contained in them, though this effect be imperceptible to us, having no electrometer nice enough to discover it. I am, &c.,

In the month of March last, I repeated Mr. GREY's experiment with the cone of sulphur and the glass; and find that, on separating these bodies, the sulphur hath hitherto ^(c) always acted as a strong negative electric. Mr. WILCKE, in repeating this experiment, observed, that if the glass vessel, into which the sulphur was poured, was covered with a coating of metal, the electrical property of the two bodies would be increased, the sulphur having acquired a stronger negative, and the glass a strong positive electricity ^(d).

* (c) Sept. 23, 1776.

(d) The stem of the glass should be varnished, or covered with cement, and the cone of sulphur (as M. LEPINAS hath directed) be provided with a glass handle, that the respective bodies may be separated at pleasure, without touching them.

I have lately seen a very neat apparatus, much smaller than that I have mentioned in this paper, made by Mr. NAIRNE, the coated plate of glass measuring only three inches in diameter. With this apparatus I made the following experiments. I insulated two of Mr. CANTON'S electrometers, A and B, and having excited the coating on the glass plate, I set upon it the plate of metal, and having permitted it to remain in that situation about half a minute, I raised it up by the glass handle, having first pressed it closely into contact, and placed it upon the electrometer A. The excited electric I placed in like manner upon B. The balls of both the electrometers diverged considerably; those of A positively, and those of B negatively. Then, removing the excited plate of wax, &c. from B, the balls closed, and opened again positively, upon the principle already explained in the preceding paper. If, instead of the brass plate, the plate of glass was excited (that is, the uncoated side of it), and placed upon the electrometer A, the balls were affected in the very same manner (differing only in the degree of power) as those I have before mentioned in my experiments with the excited uncoated plate of glass; Phil. Transf. vol. LXIV. part II. p. 407.

A variety of new experiments and observations, relative to several articles mentioned in this paper, and other new facts in electricity, particularly the electricity of chocolate, and the restoration of that property of it, when lost, by melting it, with the addition of a small quantity of olive oil, will be presented to the Royal Society, as soon as the materials are properly digested and transcribed.

XXXIII. *Account of the Iron Ore lately found in Siberia.*
In a Letter to Dr. Maty, Sec. R. S. by Petr. Simon
Pallas, M. D. F. R. S.

S I R,

Peterburg.
 Nov. 6, 1775.

R. May 16, 1776. **T**HE travels in which I have been employed, by order of our empress, since the year 1768, have interrupted the correspondence I had the pleasure to entertain with some of the Fellows of the Royal Society of London, particularly the worthy Mr. COLLINSON; and as this ingenious man, in the mean time, has left this world, I make so free as to address myself to you directly, for the leave of communicating from time to time, to the Royal Society, such observations or papers, which I am not bound to deliver to the Academy here: I would have before this observed that duty, to which the honour of being a foreign member of the Royal Society obliges me, had not the distance in which I have lived these seven years, mostly out of Europe, and the troublesome manner of travelling in these countries, together with the distractions and duties of my employment, rendered it impossible. Being now returned to a more quiet manner of living, I shall never neglect an opportunity of shewing my attachment for

the Royal Society, as well as the highest respect to that learned Body.

I have embraced the opportunity of a parcel I sent to Mr. DWYER, to offer the Society a specimen of the native iron, of which I found such a large mass in the Siberian mountains, which actually is transporting to Peterburg. I read in some foreign journals, that a short account of this mass has been published in the last volume of Phi-

LOSOPHY of our Academy; But as the contents of it, drawn from the informations I gave to our Academy in my itinerary relations, seem not to have been exact, I beg leave to give you here a faithful and fuller account of the place and circumstances, in which that memorable mass was found.

It is to be observed, that in the neighbourhood of the river Jenisei, one of the largest, that runs from the South through Siberia and to the Northern Ocean, and near which the mass of native iron has been detected; there is great plenty of iron ores, as well in the flat layers towards the Northern level of the country, where, amongst others, whole banks of *otraczoi* minerals, with scattered trees and pieces of wood turned to rich iron ore, and near the town of Jeniseisk, a rich iron ore, in the form of white clay and white sparry stones, is to be found; as also in the steep mountains, where the *Urals* dip very considerably; and ores of iron, copper, and even impregnated with gold, are found in veins and nests. On the mountains, that lie along the Eastern

side of the above-mentioned rivers, from 56° to 52° of latitude, where the highest ridge of mountains begins; iron ores are most common, and the mountains generally consist of grey or black slates and shivers, which rise steeper, or in a greater angle to the horizon, as they come nearer to the high ridge of mountains, and approach more to a level position, as they extend to the North. Some of these secondary mountains are very high, rising very often to some thousand feet above the sea surface, and most of them are covered with forests. A very rich iron ore in veins was here discovered in the year 1749, on a steep, woody mountain, about ten English miles from the river Jenisei, and 180 miles from the town of Krasnojarsk, situated on that river to the Southward, about 54° of latitude, between two rivulets known by the names of Ubei and Sifma, and running into the river on the Eastern side. This place was then visited by the Russian miners; but as there was plenty of iron ores situated much nearer to the Enbricks, this mine never was worked, though the ore contains about seventy pounds of iron in the hundred weight, being of a dark steel colour, turning red when rubbed, and in some parts endowed with a magnetic virtue. Upon the same mountain, where this mine is situated, on the North side, much below the top of the mountain, the richest natural iron lay on the very ridge, without being fitted to the rock, which is a grey stratified *gneiss*. There was only that and the neighboring mountains, no trace of mines or miners and their kilns, which are found in many other

parts of Siberia, and in which those miners, of some former and hitherto unknown nation inhabiting these parts, mostly worked upon copper ores. Nor could so large a mass ever have been formed in the small kilns of these people, which never could yield more than 50 or 60 pounds of metal at a time; whereas this mass, in its first condition, weighed above 1680 Russian pounds. It is throughout of the nature you may see in the specimen which M. DRURY will deliver to you. The iron is formed in a coarse, spongy texture, mostly pure, perfectly flexible, and fit to be worked to small tools by a moderate fire; but in a more violent one, and chiefly being melted down, it becomes dry and brittle, resolves in grains, and will no more stick together, nor extend under the hammer. In its natural state, the iron itself is incrustated with a kind of varnish, which has preserved it from rust; but, wherever this is lost, or the iron bars broken, rust comes on very readily. The cavities formed by the iron are equally filled up with a kind of *fluor*, which for the most part is of a clean, transparent, amber colour, cuts glass, has none of the properties of *scoria*, and forms, according to the hollows it fills, various roundish grains or drops, very glossy and clean, on their surface, having one or more flat surfaces. This *fluor* is extremely brittle, and thus, by cutting off any part of the mass, this substance is lost, and comes off partly in grains, and partly in form of a coarse powder of vitrescent matter. The whole mass has no regularity of form, but resembles a large, oblong, somewhat flattish pebble, and is coated on the outside

with a matter resembling some blackish, brown iron ores. This coat, however, covers not the whole mass; it is also very rich of iron, and even the transparent *fluor* yields some pounds of iron in the hundred. Whoever will consider the mass itself, or large specimens of it, will not have the least doubt of its being worked by nature, since it has no one character of *scoriaceous* matters melted by artificial fire, or commonly found among volcanos.

With regard to these, as seeming a probable place where this mass could have been formed, it may not be amiss to add the following observations. The mountains, where it was found, are part of the Northern extensions of that mighty chain of mountains which runs from West to East through Asia, and forms the natural limits of Siberia, with the Desarts of Tartary, the Mongols, and the Chinese Empire. From the river Irtysh, where the forehills and lower parts of these mountains yield, in a great many places, the richest silver ores, the chain runs generally somewhat to the North-east, and therefore extends to the East of the river Jenisei, over a much greater part of Siberia than what it did before. Its forehills are almost every where composed of rocks and *strata*, rising very steep to the horizon, and the horizontal layers are only found in the level country, in which also all kinds of fossil and petrified sea productions are very scarce, and only found in the very Northern parts of Siberia. Common flint is as scarce in Siberia as petrifications, and nothing like productions of volcanoes any where to be found. Even in some places, where hot springs are
found,

found, these seem only due to collections of *pyritae* of no great extent, and the slight earthquakes which are sometimes observed about the river Irui, and more frequently about the lake Baikal, certainly rise in the very neighbourhood of this lake and of the Noor Salitan, which gives rise to the river Irui; and about these lakes never any thing like a volcano has been heard of, nor is there one known in the Northern part of Asia, except those in Kamtschatka and the islands newly discovered between that peninsula and the continent of North America. The same may be assured of the Urallian mountains, a ridge that runs from South to East, and continues to the very Northern Ocean and Nova Semlja, being only interrupted by the Streight of Waygat. It is this ridge of mountains that makes the natural limit between Europe and Asia, and to the East of which the largest share of true remains of elephants, rhinoceroſes, and large buffaloes, is found in the banks of all the larger rivers, that run from the above-mentioned chain of mountains to the Northern Ocean, and yield ſuch remains from the places where they reach the plains of Siberia (no ſuch bones being ever found in the higher mountains) to the very Ocean; where the frozen earth of the Northern plains preſerves theſe remains of Southern animals in ſuch perfection, that when I was at Irkuzk, the head and two legs of a true rhinoceros were ſent from the river Wilui, with its ſkin and part of the tendons preſerved on them, which are now in the Muſeum of our Academy, and fully
described

the Iron Ore lately found in Siberia
described and figured in the XVIIIth volume of *Nova
Commentaria Petropolitana.*

By the very first ships that will sail from this port in
spring, I shall take the liberty to send you, for the Royal
Society, whatever I have published since 1767, and some
other curiosities for the Museum. If any particular pro-
duction, or account of natural productions from these
parts, should be wanted to the Society, I shall be ready to
serve with whatever I am able to supply.

XXXIV. *On the Crystallizations observed on Glafs.* By
James Keir, Esquire, of Stourbridge. Communicated
by G. Fordyce, M. D. F. R. S.

R. May 23, 1776. **T**HE peculiar figure of rock-crystal has been long observed. Many other substances, as spars, precious stones, pyrites, ores, metals^(a), salts, water^(b), and oil^(c), are also known to affect an uniformity of shape, when they are exposed to certain degrees of heat, cold, fluidity, and other necessary circumstances. From their resemblance in this respect to rock-crystal, they are said, when they assume their peculiar forms, to crystallize; and the regularly-shaped bodies, into which these substances concrete, are also called crystals.

(a) Native gold has been found in a crystallized form. M. ROME DE L'ISLE, in his *Essai de Crystallographie*, p. 390, says, that he has seen pieces of native gold which were eight-sided solids, like crystals of allum, and one piece which was an hexagonal plate. In Dr. HUNTER's museum, some fine specimens of crystallized native gold are to be seen. Gold may be crystallized by art also. Some rather having been poured into a solution of this metal in *aqua regia*, I observed, a few months afterwards, the gold separated from the *menstruum*, in the form of distinct polygonous prisms.

(b) The various and regular forms of the particles of snow, which is nothing else than water crystallized, are well known.

(c) The crystals, formed by cold, in the oil of sassafras, have been observed to be very beautiful, regular, hexagonal prisms.

In many substances, when broken, the parts appear to have some determinate figure. This determination of figure, or grain, as it is called, is obvious in bismuth, regulus of antimony, zinc, and all other metallic bodies, which may be broken without extension of parts; and although the ductility of gold, silver, lead, and tin, prevents the appearance of the peculiar grains, when pieces of these metals are broken, yet we have reason to believe, that, by exposing them to proper circumstances, they also would shew a disposition to this species of crystallization, as it may be called, by a further extension of that term; for Mr. HOMBERG has observed, that when lead is broken while hot, in which state it is not ductile, a granulated texture appears. Perhaps all homogeneous bodies, in their transition from a fluid to a solid state, would, if this transition were not effected too hastily, concrete into crystals, or bodies simularly figured. Instances of such crystallization have occurred to me in glass, which had passed very slowly from a fluid to a solid state; and the form, regularity, and size of these vitreous crystals have varied according to the circumstances with which their concretion had been accompanied. I send along with this paper a few specimens of this crystallized glass, together with a drawing of some of the most remarkable crystals.

The pieces of glass, marked N° 1. were taken from the bottom of a large pot, which had stood in a glass-house furnace at the time the fire was allowed gradually to extinguish. In this case, the mass of heated matter was so great that, without the addition of fuel, the heat con-

tinued long, and the transition of the glass from a fluid to a solid state was very slowly accomplished. The upper part of this glass was found to be changed into a white, opaque, or rather semi-opaque substance, resembling in colour and texture some of the white spars. Under this crust, which, in some places, was a quarter of an inch thick, and in others more, the glass was transparent, but considerably obscured, and its colour was changed from a dark green to a dull blue. In this semi-pellucid glass were dispersed many white, opaque, regular crystals, the form of which was generally that of a solid, whose side-view is represented by fig. 1. and whose basis by fig. 2. The surface of these crystals seems to be bounded by lines rather elliptical than circular, which are so disposed, that a transverse section of a crystal, that is, a section perpendicular to its axis, is an hexagon, as is shewn in fig. 3. and 4. the former of which represents a view, and the latter a plan, of that section. In the middle of each basis of the crystal, a conical cavity appears, as is shewn in fig. 1. and 2. The elliptical lines which bound the surface of the crystals seem to be occasioned by the edges of many thin plates, so arranged round the axis of each crystal, that their longitudinal diameters are parallel to that axis. Of these plates, twelve are larger, more conspicuous, and better defined, than the rest. They are placed in pairs, at an equal distance from each other, forming the six angles of the hexagonal section and basis, as appears in fig. 1. 2. 3. and 4. The intervals between the pairs of plates, that is, the areas of the triangles into which the hexagonal

hexagonal

hexagonal section is divided by these pans, and filled up partly by smaller plates affixed to the sides of the principal plates, and of each other, at an angle of 60° , and partly by a substance somewhat less opaque and darker coloured than that of the plates. The size of the contiguous and of the neighbouring crystals does not vary much, although that of crystals, found at different depths of the same pot, were observed to differ considerably. The greatest diameter of the crystals, from which the figures 1. 2. 3. and 4. were copied, was about $\frac{1}{10}$ th part of an inch, so that these figures represent the crystals considerably magnified. All the crystals are not by any means formed with the same exactness as those described; some having the hexagonal form less distinctly marked; but the regularity of most of them is so obvious, that no doubt can remain of the perfection of the crystallization.

Another kind of vitreous crystallization appears on the piece of glass marked N^o 2. which was taken from the bottom of a pot, that had been pulled out of the furnace, while the glass was red-hot. The crystals are of two kinds; those represented by fig. 5. are of the columnar form; their altitude is about one-eighth of an inch, and the diameter of their bases about one-fifth part of their altitude, their sides seem to be irregularly fluted, or cut in grooves. The other kinds of crystals, which are represented by fig. 6. 7. and 8. have bases of nearly the same diameter as the columnar ones, but their altitude is much less, being only about one-sixth part of their diameter. Their bases are bounded by lines, remarkably

ragged and irregular; but several of them shew a tendency to an hexagonal form, the regularity of which may have been disturbed by the motion of the melted glass acting upon and bending these very thin crystals, while they were hot and flexible, at the time when the pot was pulled out of the furnace.

The specimens marked N° 3. are pieces of a glass-house pot, down the outer sides of which some melted glass had run, and adhered long enough for the formation of various kinds of crystals. The inner sides also of these pieces are covered with glass variously crystallized. Some of these crystals seem to be semi-columns, of which the flat sides, or interior surfaces, are exposed to view, and are represented by fig. 9. Other crystals, represented by fig. 10. seem to consist of several semi-columnar ones, uniting together in the same plane round a common center, like broad, flat spokes of a wheel. Many of these spokes seem to become narrower as they approach the center of the wheel, and, therefore, resemble more the segments of *frusta* of cones cut along their axis, than of cylinders. But, perhaps, this appearance proceeds only from the semi-columns being so disposed near the center of the wheel, that the edge of one is laid over the edge of the contiguous semi-column, like the spokes of a fan.

In the specimen of glass, marked N° 4. which had run through a crack in a pot, and had remained adhering to the bars of the grate of a furnace, sufficiently long for a crystallization to take place; some of the crystals appear
oblong

oblong and needle-like, and others globular, or nearly of the globular form. In this piece of glafs, many of the needle-like cryftals are feen to unite round a common center; and although they have probably been prevented, by the too fudden cooling of the glafs, from concreting in a fufficient number to make complete globular cryftals, yet they fufficiently fhew the manner in which thofe, which are complete, have been formed. All the cryftallizations, hitherto defcribed, were obferved in a dark, green window-glafs, made at Stourbridge, and called Broad-glafs. This glafs is compofed of fand, kelp, calcareous earth, and lixiviated vegetable afhes.

Crystallizations frequently occur alfo in the glafs of which common bottles are made, the materials ufed in the compofition of which are nearly the fame as thofe above mentioned for broad-glafs, with the addition fometimes of the *scoria* of iron furnaces. Of this kind is the fpecimen marked N^o 5. in which the cryftals are not involved in a medium of transparent uncryftallized glafs, for the whole piece is an opaque, cryftallized fubftance; but they are prominent from the furface of the mafs. The form of the cryftals is that of the blade of a two-edged fword, whole point is truncated. In no other glafs have I feen fuch perfect cryftals as in thofe two kinds above mentioned, broad and bottle-glafs, which being more fluid and lefs tenacious, when melted, than any other, the minute particles of which cryftals confift, more eafily concrete, and apply themfelves to each other, with lefs refiftance from the medium. Perhaps alfo the
greater

greater proportion of calcareous and other earthy particles may dispose these glasses to crystallize more than others, which contain a larger quantity of saline and metallic fluxes.

Flint-glass, when long exposed to a dull red heat, acquires a cloudiness, which, probably, proceeds from a number of small white particles, concentered by means of crystallization; but these crystals are too minute for observation. I suspect also, that the opaque whiteness, given to glass by arsenic, is the effect of a crystallization, to which this substance disposes certain kinds of glass; for the opacity given to such glass by arsenic, being greater than the opacity of the arsenic itself, cannot be communicated to a large proportion of transparent glass, merely by the mechanical interposition of this opaque, and sometimes only semi-opaque substance.

Mr. REAUMUR has observed, that some kinds of glass, by long exposure to certain degrees of heat, acquire a white opaque crust on their surface; and that this change of colour and texture, by a longer continuance of the heat, penetrates farther, till at length the whole substance of the glass is converted into a white, opaque body, which, from some supposed resemblance to porcelain, has been distinguished by the name of REAUMUR'S porcelain, but is really nothing else than glass indistinctly crystallized.

Some of the properties of glass are considerably changed by crystallization; its transparency is destroyed, and it acquires an opaque or semi-opaque whiteness; its density is

increased, for the density of a piece of crytallized glafs was found, by experiment, to be to that of water as 2676 to 1000; whereas the density of a piece of uncrytallized glafs, which had been contiguous to the former, and consequently had been composed of the same materials, and exposed to the same heat and other circumstances, was to the density of water as 2662 to 1000. The brittleness of glafs is diminished by crytallization; for crytallized glafs is less apt to crack by change of heat and cold.

Crytallization is always accompanied or preceded by an evaporation of the lighter and more fluid parts of the glafs; for I found, that, by exposing a piece of transparent glafs till it was entirely crytallized, one fifty-eighth part of its weight was lost by evaporation: and I am induced to believe, from other trials, that glafs, which contains too large a proportion of saline fluxes, is less capable of crytallizing than other harder glassess, till it has lost its superfluous quantity of such fluxes by evaporation. A doubt may therefore arise, whether the change of properties, induced by crytallization, be merely the effects of altering the texture, that is, the arrangement of the minute integrant parts of glafs; since this change is always accompanied with a loss of the lighter parts of the crytallizing substance. But, although a superfluous quantity of saline or other fluxes may impede the crytallization, yet, that the change of properties, induced by crytallization, is principally or solely the effect of an alteration of texture, is evident from this observation; that a piece of crytallized glafs, when exposed to a heat considerably

considerably more intense than is sufficient merely for its fusion, and afterwards hastily cooled, loses all its acquired properties, and is again reduced to the state of transparent brittle glass, which, however, by means of the evaporation it has sustained of its lighter and more volatile parts, is rendered considerably harder, denser, and less fusible, than it was before the crystallization.

Many analogous instances might be adduced to shew how much the properties of bodies depend merely on the different arrangements of their integrant parts, or on their modes of crystallization. Thus, for instance, cast-iron and steel, when cooled suddenly, acquire a much finer grain or texture than when annealed, or slowly cooled, and are also more hard, elastic, brittle, and sonorous. From the above description of vitreous crystals we learn, that very different crystallizations occur in the same kind of substance exposed to different circumstances; and even that sometimes differently-shaped crystals are found in the same piece of glass; in which case, the circumstances must have been the same. Perhaps, indeed, the difference, observable in the shape of the crystals in the same piece of glass, may only mark the different periods in the progress of crystallization; for the crystals represented by fig. 6. 7. and 8. which are found in the same piece of glass as those represented by fig. 5. do chiefly differ from these in their altitude; and perhaps the latter kind may have been composed of a number of the former uniting by their bases. The wheel-like crystals also, fig. 10. seem to consist of several
of

of the semi-columnnar one, arranging themselves round a common center like the spokes of a wheel. The globular crystals in the specimen N° 4. have been already observed to consist of many needle-like crystals, converging to one central point.

Does not this discovery, of a property in glafs to crystallize, reflect a high degree of probability on the opinion, that the great native crystals of *basaltes*, such as those which form the Giant's Causeway, or the pillars of Staffa, have been produced by the crystallization of a vitreous *lava*, rendered fluid by the fire of volcanoes?

This opinion is further confirmed by the following considerations. The prismatic and other regularly-shaped *basaltes* have been almost always found to be accompanied with *lava*, pumice-stones, and other vestiges of the fire of the volcanoes, whenever they have been carefully examined by intelligent naturalists, as has been shewn by M. DESMARETS, in his Memoir on the *Basaltes* of the province of Auvergne, in France; *Mem. de l'Acad. des Sciences*, 1771. Basaltic columns have even been discovered, according to the same author, among the productions of volcanoes now existing, as of those of Mount Etna and of the Isle of Bourbon.

2. The substance of which these basaltic masses consist, is generally of the same nature and appearance as the neighbouring and adjoining *lava*. It is generally compact, fusible, and of various degrees of hardness, probably according to the matters of which the vitreous mass was compounded. M. DESMARETS has further

observed, that the prismatic *basaltes* of Auvergne is actually a continuation, and generally the termination, of a current of *lava*.

3. Although the variety of the forms of the crystals, in the same kinds of glass, and even in the same piece of glass, which has been already remarked, sufficiently shews the uncertainty of any inference drawn from a similarity of shape; yet it may not be improper to mark the analogy, in this respect, between the basaltic and vitreous crystals. The columnar or prismatic form is known to appear most generally in the crystallized *basaltes*. Of this form also are evidently the crystals represented by fig. 5. The semi-columnar, vitreous crystals, fig. 9. seem to be analogous to the no less singular basaltic semi-columns observed in the Giant's Causeway by Bp. POCOCK (Phil. Transf. vol. XLVIII.); which, he says, were exactly like hexagonal columns cut in two. M. DESMARETS has observed, in the province of Auvergne, great quantities of spherical and ellipsoid basaltic concretions, which were formed of polygonal columns, rather pyramidal than prismatic, converging from the circumference to the center. These seem to be perfectly analogous to the vitreous globular concretions which have been above observed to be composed of oblong crystals, arranged in a similar manner. The same author also observed, in the same province, regularly-shaped tables or plates of *basaltes*; of which, he says, assemblages were accumulated in all directions. We have shewn, that the crystals represented by fig. 1. 2. 3. and 4.

are

are really assemblages of plates or tables, disposed in every direction round a common axis.

Lastly, The stone on which the columns of *basaltes* generally rests, and which sometimes also is supported by these columns, being of the same nature and texture as the columns themselves, seems to be a mass irregularly crystallized, analogous to the irregularly-shaped masses in the specimens of glafs N° 1. and 2. which evidently consist of a similar substance as the neighbouring crystals, and seem to have been composed of a number of these crystals indistinctly united; for the peculiar figures of crystals are distinct only when they are insulated, or when they are separated from each other by a pellucid or differently-coloured medium. A medium of this kind appears between the vitreous crystals, and is nothing else than the more fluid parts of the glafs, which longer resist the concretion, but which, by a further continuance of the heat, would have become, with the parts already crystallized, one uniform, white, opaque substance, without any interposition of transparent glafs, or distinction of crystal, except on the surface, as in specimen N° 5, in which the crystals stand prominent from the indistinct mass, and unenveloped in any medium, in the same manner as the basaltic crystallizations appear standing above the mass of stone or *lava* which supports them.

Further observations on the basaltic and vitreous crystals may probably suggest more instances of analogy

between these two substances. No just objection can be drawn against this analogy from the magnitude of the former compared with the minuteness of the latter: for the difference of size between the small vitreous crystals and the stupendous basaltic columns, which support mountains, islands, and provinces, is no more than is proportionate to the difference usually observed between the little works of art and the magnificent operations of nature.

Fig 2

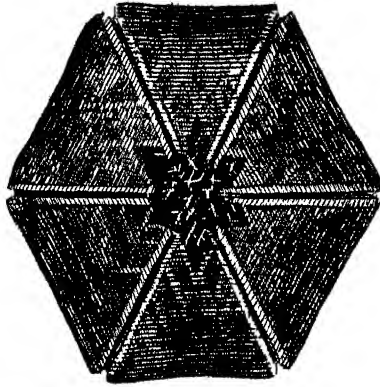


Fig 4



Fig 9



Fig 7

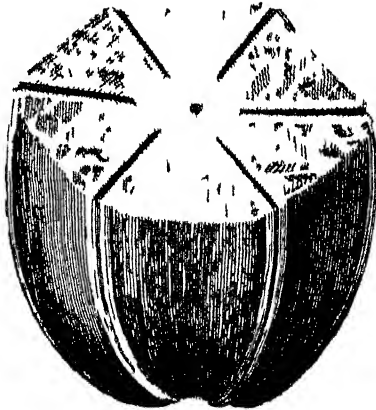


Fig 8



Fig 3
p. 6

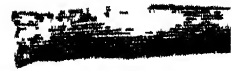


Fig 5

Fig. 4.

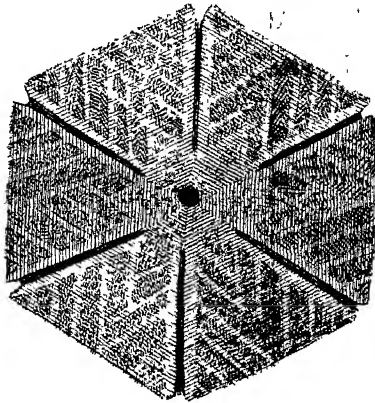


Fig. 10.

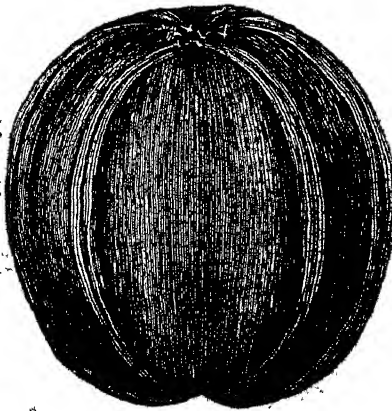
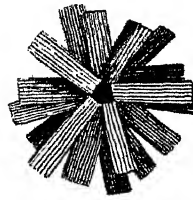


Fig. 1.

XXXV. *A Belt on the Disc of Saturn described in an extract of a Letter from Mr. Messier, F. R. S. to Mr. Magellan, F. R. S. Dated Paris, May 29, 1776.*

R. June 6, 1776. **I** HAVE observed, since the 14th of May, a belt of a fainter light on the body of Saturn, opposite to the part of the ring behind the planet. It is pretty broad, and almost as distinct as those of Jupiter. It was with a very good achromatic of three foot and a half, made by Mr. DOLLAND, that I discovered this appearance. I wish you would communicate it to the astronomers, because those who are furnished with better instruments may, perhaps, see some inequalities in this belt of Saturn, and so the time of the planet's revolution on its axis may be better ascertained than it is at present. Mess. JOHN and JAMES CASSINI seem to have been the only astronomers who discovered this phenomenon about the end of the last century.

XXXVI. *An Account of some poisonous Fish in The South Seas. In a Letter to Sir John Pringle, Bart. P. R. S. from Mr. William Anderson, late Surgeon's Mate on board His Majesty's Ship the Resolution, now Surgeon of that Ship.*

TO SIR JOHN PRINGLE, BART. P. R. S.

S I R,

Resolution,
Deptford, April 23, 1776.

R. June 6, 1776. **I**N compliance with your request I have sent you the few notes which I had taken of the cases of some of our ship's company, who, on our late voyage to the South Sea, had experienced the bad effects of eating certain fish of a poisonous nature. I was, perhaps, less solicitous about remarking the minute circumstances attending their illness, as I then believed it was a disorder well known in the West Indies, having frequently heard of people being poisoned, as it is commonly expressed, by eating some particular kinds of fish; but, as far as I have been able to inquire since of those who have been there, or from books, I do not find that any tolerable account either of the disease, or of the means of curing it, has been made public.

This being the case, it is almost needless to say, that in treating the disorder we could have no method founded
on

on experience to pursue, and therefore were obliged to palliate the symptoms, from the analogy they bore to those that occur in other diseases. From a supposition, that some of the matter, which caused the illness, might lie indigested in the stomach, Mr. PATTEN, the surgeon, ordered some warm water to be drunk, in order to make the patients vomit; which effect it had with some of them, and they were a little relieved by it. After the *nausea* had ceased, he gave some weak portable soup, as a diluent; and for the most troublesome symptom, *viz.* the heat on the surface of the body, he prescribed a sudorific julep, whereof the active ingredients were the antimonial wine and *spiritus mindereri*. This, in some measure, had the desired effect, as it brought on a breathing sweat, which, for the time it continued, abated the violence of the pains. No other medicines were used, excepting some purging salts, for preventing inflammation, in two or three, whose mouths and throats had been more particularly affected. Their diet consisted chiefly of tea, sago, and portable soup.

I have avoided saying any thing about the manner in which the poison operates, as the instances have been too few, to draw any certain consequences from them. I would only observe, that its action may be such, as to affect and deprave some of the organs of sensation, without much irritating the first passages; because in all the patients the disorder of the stomach and bowels had long ceased before the other symptoms went off. And I was confirmed in this opinion by a circumstance which afterwards

afterwards happened to Captain COOK, who, having eaten a small piece of the liver of another kind of fish (a *tetraodon*) was not sensible of being hurt by it, till waking in the night, and calling for a draught of water, he neither could feel the vessel with his hands, nor was sensible of its weight when he grasped it. On the other hand, it was remarked, that some of the other gentlemen, who had likewise eaten of that fish, had also a vomiting and looseness. The difference, perhaps, depended on the quantity taken into the stomach, and the particular constitution of the person.

I shall only add here, that, having been favoured by Mr. BANKS with a sight of his drawings, I find the *Sparus Pagrus* of LINNÆUS to be the fish which that eaten by our people most resembles. It is probable, that it is the same fish that QUIROS found to be of a poisonous nature; but at the same time I must observe, that it may well be doubted, whether this species is always poisonous, as our men ate another of the same sort about a month after, without being affected by it. I am, &c.

SATURDAY, July 23, 1774, on board His Majesty's ship the Resolution, off the Island Malicolo, in The South Sea, three fish, of the same species, that had been caught, being dressed for dinner, affected all those, who ate of them, in an uncommon manner; but five persons, who had eaten of one of them, were more severely attacked than the rest. Immediately after eating, nothing was felt but some uneasiness (or such pain as follows

from swallowing any acrid substance) in the mouth and throat. About two in the afternoon, some felt an uneasiness in the stomach, with an inclination to vomit; but it was near the evening before those who suffered most were affected. The symptoms at first were universal lassitude and weakness, followed by a retching; and in some, by gripings and looseness. To these succeeded a flushing heat and violent pains in the face and head, with a giddiness and increase of weakness; also a pain, or, as they expressed it, a burning heat in the mouth and throat. Some had the mouth affected in such a manner, that they imagined their teeth were loose; which might really be the case, as a considerable spitting attended this symptom. The pulse all this time was rather slow and low. The retching and looseness did not last long; but the pain and heat of the head were extended to the arms, hands, and legs. The patients continued in this manner all the night, but with some intervals of ease. Towards the morning, the pains, especially those in the legs and arms, but more particularly about the knees, were severer than before. These would sometimes remit and frequently shift, or be more violent in one place than in another. Sometimes the pain would remove suddenly from the legs, and fix in the head; the palms of the hands were hot; and the fingers, legs, and toes, felt often as if benumbed: nay, the whole limbs became in some measure paralytic; the sick person being unable to walk unless supported. Although there appeared no swelling in the face, it might be observed

to have a sort of shining or gloss upon it; and the patient sometimes imagined his nose was grown to a great size.

24th, In the forenoon they continued much the same; but some, after sleeping, were rather easier; and one had a copious spitting, which, however, gave him no relief, for at noon the pains in his limbs ceasing were removed to his head, which they affected violently with a sense of throbbing and great heat; nor were there any of the patients, but this one, who complained much of thirst during the illness.

Another, in particular, had the pains in his knees so increased, that they made him cry out. The uneasiness at the stomach and heat of the throat in all had nearly ceased. When the *mucus* about the *fauces* was forced away by straining, it felt hot, and left the same sensation about the throat for some time. In the afternoon, most of them grew much easier, but continued weak; and two, who now seemed better than they had been before, complained of heat and soreness in their hands and feet.

25th, All five had rested tolerably in the night; but complained of weakness and soreness of the mouth, with heat in the hands and feet. One, who had been rather worse than the others, still had a considerable spitting.

26th, All continued better, but the pains were not entirely gone. The great weakness, with heat in the hands and feet, were still general complaints, and the soreness in the mouth remained in some. The one mentioned as having a large discharge of *saliva*, continued spitting;

spitting; and another began to have the same complaint, though in a less degree.

27th, One of the five had no other complaint but a disagreeable sensation on rubbing his skin in any part of his body. Another, as yesterday, mentioned a slight giddiness, weakness, &c. A third, who had been the worst of all, and who had taken some purging salts, was much better. The other two, who had seemed so well the day before, became worse at night; one of them, who before had told me of violent pains in his knees, had a return of the same symptom; the other had pains in his legs, and an universal uneasiness.

28th, They were all considerably better, except one, who had been seemingly well the night before, but complained now of much weakness, and flying pains in his limbs.

29th, All of them mended, but still complained of wandering pains in the limbs, weakness and heat in their hands and feet.

30th, Continued as yesterday. At times they seemed quite well; but, in the morning, they complained of more weakness than in the preceding evening; and the pains, which generally appeared to be almost gone in the day, returned at night.

31st, All were better, but not without some slight pains and a weakness in the morning; and some still felt a disagreeable heat in their hands and feet, and others in their mouth.

August 1, One had no complaint. The rest not altogether free from pains, and a sense of weariness or weakness.

2d, All were pretty free from pain; but one had still too much heat in his hands.

3d, All recovered; some trifling pains and a little weakness excepted.

These notes I kept of the five who had eaten of one fish. With regard to those who had eaten of the other two fish, they were not so severely handled. No signs of illness appeared amongst them till night; some had then a *nausea*, retching, and some loose stools. Others felt only pains in the backs, arms, and legs, as in a rheumatism; but it was observed, that in all, the face was more or less affected with an uneasy sense of stiffness. All these, though at first slightly ailing, and though the symptoms were later in coming on, had this circumstance attending them, that they were all worse next day in the evening.

On the 25th, they continued much the same as on the day before in the afternoon; but, towards night, they grew somewhat better. They had pains in their arms and legs, with a weakness and sense of heat on the surface of the body.

26th, All complained in the same manner; and, though the pains were less, they continued, often shifting suddenly from one place to another, and the weakness rather increased.

27th, Two of them, who had suffered least, remained much the same. The others were sensibly better.

28th, Most of them had wandering pains in their arms and legs, but were, in other respects, visibly better. One who, for the first two or three days, had scarcely complained, said, that this day he had a head-ach and pains in his legs and arms.

29th, None of them yet perfectly recovered; all complaining of lesser pains in their legs, arms, or back.

30th, All continue much the same.

31st, All better, but not altogether free from pains and weakness.

Aug. 1st, Some were recovered, and the rest better.

2d, All of them were pretty well; and they continued so.

Several of our dogs, who had eaten of these fish, and especially of the guts and bones, seemed affected in a higher degree than the men. Their illness might, perhaps, have begun in the night, but was not perceived till next morning. One, in particular, had violent retchings, a *paralysis* (especially of the hind legs), lay down, rolled, howled, and shewed other signs of great pain. More *saliva*, or froth, hung about his mouth; and, when fatigued with struggling, would fall down, and seemingly breathe with great labour. Two of them continued in the same way all day; though others that had eaten, as much appeared but slightly affected, their principal symptom being only weakness in the hind legs. It was

observed, that most of them were troubled with a swelling of the *penis*.

One lay the first day, all the afternoon, without being able to move, but groaned perpetually, and lay in appearance in great anguish.

Next day, they seemed all somewhat better, except one, to whom tobacco-juice had been given the day before, to make him vomit: he died in the afternoon.

On the third day, though the dogs were more free from pain, yet they continued almost motionless, nor did they begin to eat.

On the fourth day, they were all much better, the worst of them running about, and eating his victuals.

On the fifth, all of them seemed recovered, one excepted, who had not been so soon or so violently attacked as the others. This remark was made of the men likewise; to wit, that those who were more slightly and later attacked continued ill as long as the others. Some days after, the same dog became so paralytic in his hind legs, that he could not stand; but afterwards he recovered the use of them pretty well.

A hog, who had eaten of the offals, died next morning; as did a perroquet on the second day, which had got from his master some of the boiled fish. It will be proper to add, that another hog died, which had fed on the entrails of that fish whereof Captain cook had eaten a bit of the liver; and that a dog, which about three weeks after had eaten of a fish of the same species, died, after a lingering illness, of the same nature with that which had affected others of his kind.

XXXVIII. *Experiments on Ignited Substances.* By Mr. John Whitehurst, in a Letter to James Stuart, Esquire, F. R. S.

TO JAMES STUART, ESQ.

S I R,

London,
Nov. 29, 1775.

R. May 23, 1776. **T**HE experiments of Mr. BUFFON upon ignited bodies seem to prove, that, when heated to the degree he mentions, they are more ponderous than when cold. The experiments which I have made on heated metals, suggest a different idea, and contradict the fact he relates; so that I am induced to believe, that some circumstance, not attended to, has introduced a mistake in the relation this learned philosopher has published as the result of his inquiry.

His experiment stands thus recorded (Suppl. Nat. Hist. vol. II. p. 11.): a mass of iron, after receiving a white heat, weighed 49 lbs. 9 oz.; when restored again to the temperature of the atmosphere 49 lbs. 7 oz. Hence he concluded, that the igneous particles, contained in the heated iron, increased its absolute weight 2 ounces.

My experiments are as follows: 1st, One pennyweight of gold, made red-hot, became apparently lighter; but, when restored again to the temperature of the atmosphere, its former weight was perfectly restored.

2d, One pennyweight of iron, heated as above, was also apparently lighter; but, when it became cool again, its weight was visibly augmented.

It is now several years since I made these experiments; but I well remember to have repeated them several times, and that the results were always the same. It may be necessary to remark, that the beam used in these experiments was sensibly affected by the $\frac{1}{10000}$ th part of a grain; and likewise, that each of the metals was heated upon charcoal, by means of a candle and a blow-pipe, and both were brought nearly to a state of fusion.

It seems needless to observe, that the apparent levity of the gold and of the iron, when hot, was owing to the ascent of the rarified air above the scale, and to the tendency of that underneath to restore the equilibrium of its pressure. The increase of weight in the iron might probably arise from its having, in some degree, acquired the property of steel, by means of the flame and charcoal.

I am at a loss to account for the fallacy which seems to have attended M. BUFFON'S experiment; but it seems probable, that the heat of the mass of iron employed by him, had a greater effect on that arm of the beam from
which

which it hung than on the other, which being less heated, would consequently be less expanded; and this difference of expansion might produce the error in M. BUFFON's account of the weight of heated iron.

If these observations afford you any satisfaction, it will give me pleasure.

I am, &c.

XXXIX. *An Account of a Suppression of Urine cured by a Puncture made in the Bladder through the Anus; being an Extract of a Letter from Dr. Robert Hamilton, Fellow of the Royal College of Physicians at Edinburgh, and Physician at King's-Lynn, in Norfolk, to Sir John Pringle, Bart. P. R. S.*

On May 23, 1776. **I** WAS sent for on the 25th of March, 1774, to visit JAMES WILKINSON, of about thirty-one years of age, the son of a farrier in this town, who had laboured under a suppression of urine for three days.

The account this man gave of himself was, that he had been fourteen years a soldier in a regiment of foot, but had been discharged the preceding year as unfit for service, on account of this disorder, to which he had been long subject, and which was supposed to originate from a stone in the neck of his bladder; that, about twelve years before his discharge, he had contracted a *gonorrhoea*, which had been badly cured; that, from that period, he had become afflicted with frequent suppressions of urine, and that every return had been of longer duration than the former; that when the suppression had continued some time, he had been used to go to the Regimental Hospital, and to stay till it was removed; and that,

bleeding,

bleeding, purging, and the warm-bath had been the means used for his relief. He added, that the catheter could never be introduced into his bladder from the first attack; that the first signs of relief were a few involuntary drops of urine issuing from the *urethra*, after which followed a small thread-like stream, which continued to run until the bladder was a little more than half emptied; for he did not think that his urine had at any time been perfectly discharged, during the thirteen years he had been afflicted with this painful disorder. He concluded with telling me, that in other respects he had enjoyed good health; and that the present suppression had been brought on by hard riding on a journey he had undertaken to go express.

This man was in a very distressed condition. His bladder was distended to an enormous size, resembling the gravid *uterus* at that late period of gestation, when the *fundus* reaches above the navel; to such a degree was it dilated by the repeated suppressions of urine for so many years. His pulse was small and quick; he had been troubled with a hiccup for many hours, and had vomited every thing he had taken from the first day of his illness. His head and face were emphysematous from straining; and the cellular membrane was so much inflated, that he could not open his eye-lids. He was attended by two surgeons, who had used bleeding, opiates, clysters, fomentations, and the warm-bath, without any benefit; and they had repeatedly attempted to introduce catheters of different sizes, without success; that instrument, having passed without resistance to the neck.

of the bladder, met there with an obstruction not to be overcome. Under such pressing circumstances no time was to be lost. The patient's life was in imminent danger, either from a rupture, or a gangrene of the bladder. The approach of the latter was soon to be apprehended, from the hiccup and smallness of the pulse. There was no alternative left; the bladder must be speedily emptied by an aperture somewhere, or the patient must perish. The surgeons, therefore, went to prepare their instruments for making an opening into the bladder. Reflecting, in their absence, on the most eligible method of performing the operation, I was in some degree of perplexity; for I did not much approve of any of the methods recommended by the best chirurgical writers; either that of making a perforation above the *os pubis*, in the place where the higher operation of lithotomy is performed; or that of the puncture *in perineo*. Whilst I was considering what was to be done, the mother of the patient told me, that she had several times that day attempted to give him a clyster, but had not been able to introduce the pipe, by reason of a large substance low in the gut near the fundament, which stopped up the passage. It immediately occurred to me, that the obstructing body could be no other than the distended bladder, which, having filled the *pelvis*, pressed downwards where there was the least resistance towards the *anus*, as well as upwards into the *abdomen*. From this circumstance I was led to think, of discharging the urine by a puncture into the bladder, with a trocar introduced by the *anus*. I conceived that this method would have advantages superior
to

to any other that I had heard of, for simplicity, ease, and safety. The finger could guide the point of the instrument to the very spot of the bladder to be pierced. The coats of the *intestinum rectum* and bladder, and the intervening cellular membrane, were all that were to be perforated, and they were now pressed so closely together, that there could be no more art required than in piercing any simple bag of water. When the surgeons returned, I told them what had occurred to me concerning the descent of the distended bladder (for I had not yet examined it) and its pressing the *rectum* downwards; I represented to them the hazard and difficulty attending the operation hitherto in use, and proposed this method of perforating the bladder with a trocar introduced by the *anus*. They readily acknowledged the advantage of such a practice, and agreed to give it the preference. We examined with a finger *in ano*, and felt a large round tumor, a very little way within the orifice, pressing the anterior side of the *rectum* downwards, and pushing the *anus* and *perineum* considerably outwards. The gut itself was loose and empty; and through its relaxed sides the tumor, which was evidently the bladder, was distinctly felt stretching every way, completely filling the *pelvis*, and feeling like the membranes which contain the waters of a woman in labour, thrust into the dilated *vagina*. I described the manner in which I thought the puncture should be made; and, as I imagined that I could better execute what I had myself conceived than another person, I offered to do it; which being readily assented to,

the operation was performed in the following manner. Having placed the patient on his back on a bed, with his breech projecting a little over the side of it towards the light, and his legs bent into the position they are placed in for the operation of lithotomy, and held by two assistants, a trocar of the middle size, with its point guarded by the extremity of the fore-finger well oiled, was introduced into the *anus*, until the tip of the finger reached the anterior part of the tumor; when the finger being a little withdrawn, and the point of the instrument brought into contact with the tumor, it was plunged into it, in a direction parallel to the axis of the bladder, in an erect posture; and the perforator being pulled out, the water immediately followed. A straight catheter was quickly introduced through the canula into the bladder, lest, as it collapsed and shrunk upwards as the water was discharged, the canula should prove too short, for its shell was then close to the *anus*. The canula was then slipped out of the aperture as far as the rings of the catheter, as being no longer of use, and the catheter remained in the bladder until the urine was all drawn off; during which time, that instrument was moved different ways, to search for the stone which was supposed originally to have occasioned the disorder; but none was to be found. The water being discharged, the catheter was taken out, and the patient put to bed. The parts were repeatedly fomented; and a draught, with half a drachm of nitre and 25 drops of laudanum were given, and two more of the same kind were ordered for the night.

26th, He had had a very good night, and had made water five or six times through the aperture made by the trocar. He said, that as soon as the bladder had collected a certain quantity of urine, he had felt an inclination to make water; that then sitting on a chamber-pot or bed-pan, and straining in the usual way, the urine had rushed out at the aperture *per anum* in a stream; and that none had passed by the *urethra*. Not contented with his account of the matter, I desired him to make water in my presence, and was witness to this curious and extraordinary power of retention of the urine in a wounded bladder, and of discharging it at pleasure through an artificial passage. The emphysematous swelling of his head and face was almost gone. He was directed to drink the pectoral decoction with the addition of some marsh-mallow root, sweetened with manna, and acidulated with orange or lemon-juice; and the nitrous opiate was repeated.

27th, He complained of a fulness of his belly, probably owing to his not having had a stool. He still made water through the trocar-aperture as before, and whenever he pleased; but now he began to perceive a little urine come by the *urethra* at the same time. We did not choose to order a clyster, lest the gut should be injured by the pipe, but to wait the effect of the manna, which, with the night-draught, was still continued. The resolution of the inflammation being now begun, as was conjectured by the urine finding its natural passage by the *urethra*, a bougie was introduced beyond the stricture at the neck of the bladder, and to very good purpose.

28th, He was much better, he had had some stools, and had passed most of his urine by the *urethra*.

30th, No water had issued through the aperture either this morning or the preceding day; the whole, though in a small stream, had flowed by the *urethra*. He complained of a forenefs *in ano*.

31st, This day he felt that forenefs only when he went to stool. The stream of urine by the *urethra* was still small.

April 6th, The puncture through the *rectum* and bladder appeared to be quite healed. The urine was discharged in a tolerable stream, the passage being, as he observed, much wider than it had been for thirteen years before.

He continued the daily use of the bougies; and, being sensible of the great benefit he had received from them, willingly persevered in their use, until the stricture was so much lessened, as to permit a free discharge of his water, and by these means he obtained a complete cure: for, in two months after, he left the town in every respect well. It was remarkable, that, during the progress of the cure, no urine was perceived to ooze involuntarily through the opening; but it was always retained until the patient, prompted by a fulness of the bladder, made his water, as has been related.

Having given as accurate an account of this case as I am able, I must now, SIR, in justice to myself, beg leave to assure you, that I had neither heard, nor read, of any method of perforating the bladder, similar to that which I have related, before you kindly informed me in your

last letter, that it was not a new operation; and that I should find an account of it in POUTEAU's *Melanges de Chirurgie*. I have procured the book, and have read the paper with great satisfaction, and am much obliged to you for your intelligence. I do not presume to claim any merit from a thing which took its rise from mere accident. The obstruction which the pipe met with when the patient's mother attempted to give him the clyster, suggested the method of piercing the bladder by the *anus*; and I am perswaded, that the same thought would have arisen, on a like occasion, in the mind of any thinking man. And I beg you would believe, that I have no small satisfaction in finding it occurred to M. FLURANT, the ingenious author of the paper on that subject, in that publication, from a circumstance, which, though not exactly similar, amounted nearly to the same thing. This surgeon having introduced his finger into the *anus*, to examine the state of the bladder, in order to perform the puncture *in perinæo*, found it so round and tumid, and so much within the reach of his instrument, that he thought he could perforate it with safety in that place; and, from a little reflection on the structure of the parts, was convinced of the expediency of operating in this manner, in preference to any other.

Before I quit this subject, I think it proper to acquaint you, that I have found a composition of calomel and opium, in large doses, the best internal remedy for suppressions of urine, in general; and that I have repeatedly seen this medicine succeed after the usual means have failed. I am
convinced,

convinced, from these trials, that the principal or specific efficacy is in the calomel, as large doses of opium alone, or joined with camphire, have proved unsuccessful. I am so well satisfied of the advantages of this practice, that, if called early in the disorder, I direct ten grains of calomel with two grains of solid opium, made into a bolus with any conserve, to be taken immediately, and repeated in six hours. I have seldom occasion to order a third dose, the patient being generally relieved by the second, if the first has failed. I did not administer it in the case here related, because the alarming situation the patient was in when I came to him required the bladder to be emptied without delay.

XL. *Observations made during the late Frost at North-hampton.* By A. Fothergill, M. D. Communicated by William Henley, F. R. S.

TO THE REV. DR. HORSLEY, SEC. R. S.

REV. SIR,

R. June 27,
1776. **T**HE following letter, from my learned friend Dr. FOTHERGILL, contains, in my opinion, many very curious experiments and observations; and, though it was not intended for that purpose, I cannot but think it well deserving the notice of the Royal Society. I have, therefore, SIR, taken the first opportunity to put it into your hands.

And am,

Yours very sincerely,

W. HENLY.

DEAR SIR,

Northampton,
May 3, 1776.

ACCORDING to my promise, I now proceed to give you a short account of some observations and experiments concerning the late severe frost. As some of the phenomena appeared to me not a little surprizing, I minuted them down at the time they occurred.

Jan. 27th, The great quantity of snow which had continued falling almost every day for three weeks, had, for these five or six days past, rendered the roads impassable; and the post, both upwards and downwards, was stopped, the snow being drifted from six to ten feet deep or upwards. This morning the frost became suddenly very severe; the wind full East, accompanied with snow. The barometer stood at $29\frac{3}{4}$. A thermometer, according to FAHRENHEIT's scale, which hung in my parlour, where there was a good fire, stood at 33° , that is, only 1° above the freezing point. After it had been suspended a quarter of an hour on the Chinese palisades before the street-door facing the South, it sunk to 20° , that is, 12° below the freezing point. At five o'clock the same evening, it fell to 16° . At this time eggs in the market cracked in the womens baskets, and appeared in a coagulated state, of the consistence of bees-wax. This evening was placed on my garden-wall, facing the East, half an ounce of each of the following liquors in a cup; *viz.* lemon-juice, vinegar, and red port-wine.

Jan. 28th, This morning, at eight o'clock, the barometer stood at 30. The thermometer at 12° , that is, 20° below

below the point of freezing; wind Easterly; the atmosphere clear and serene, but piercing cold. The three liquors were reduced to a solid cake of ice. This night, about eleven, were placed on the same wall the following liquors; viz. spirit of *Mindererus*, volatile spirit of *sal ammoniac* of both kinds, mild and caustic, dulcified spirit of nitre, red port-wine, and French brandy.

Jan. 29th, Barometer $29\frac{2}{10}$; thermometer at 11° , that is, 21° below freezing; the Easterly wind excessively keen and piercing. The roads which, at great labour and expence, had just been cut through for carriages to pass, were again this morning, though no fresh snow had fallen, completely drifted up. These liquors also, to my great surprise, now shewed evident marks of freezing. They were suffered to remain, and two more cups were placed near them, with highly rectified spirit of wine and vitriolic ether. At a little distance was placed, in a frigorific mixture, consisting of a combination of the vitriolic acid with snow, about an ounce of crude quicksilver in a phial.

Jan. 30th, The morning clear, but intensely cold; wind S.E.; barometer $30\frac{1}{10}$; thermometer sunk to 9° , that is, 23° below the freezing point; a degree of cold which, I apprehend, has been but rarely experienced in this climate, being $3^{\circ}\frac{1}{2}$ below that of the remarkable frost in the year 1739. On examining the liquors on the garden-wall I found, to my astonishment, all of them, except the spirit of wine and ether, perfectly congealed: the first time I had ever seen these liquors in a solid form.

Being desirous to see the effect of a high degree of artificial, added to the natural cold that now prevailed, the thermometer was immersed into the frigorific mixture; but though it sunk the quicksilver, in a few seconds, into the bulb of the thermometer, yet the result was by no means adequate to that of the experiment of Professor BRAUN at Petersburg: for although the quicksilver in the thermometer, and that in the phial, contracted a film on the top, yet it remained quite fluid below.

Jan. 31st to Feb. 1st, The barometer at 29; the thermometer only at 16° , that is, 17° below the point of congelation; the atmosphere serene and pleasant.

Feb. 2d, Wind S.; barometer $29\frac{1}{2}$; a warm, misty morning, succeeded by a pleasant, spring-like day, ushered in a very mild and agreeable thaw, the thermometer from 9° being got up to 40° ; so great was the change of temperature in so short a space of time! And it seems worthy of observation, that the epidemic cold, which had prevailed universally during the preceding mild season, suddenly disappeared in the late intense frost; but now began to re-appear, together with rheumatic affections and other diseases of the former period.

I am, &c.

XLI. *An Account of the Magnetical Machine contrived by the late Dr. Gowin Knight, F. R. S. and presented to The Royal Society, by John Fothergill, M. D. F. R. S.*

TO THE PRESIDENT AND FELLOWS OF
THE ROYAL SOCIETY.

GENTLEMEN,

R. June 27, 1776. **B**Y being left executor to your late worthy member GOWEN KNIGHT, M. B. a very extraordinary magnetic machine of his contrivance, and which had cost him much labour and expence, came into my possession. This, I thought, might not be unworthy of a place in your repository; and I therefore desire your acceptance of it, as a monument of Dr. KNIGHT's very singular abilities, and of my regard to the purposes of your institution.

I must, however, inform you, that this machine, which, by the annexed figure and its explanation, may be observed to consist of two parts, is by no means so strongly magnetical as it was at the doctor's decease. Not long after this event, it was necessary to remove this apparatus from
his

his apartments in the British Museum. One of these parts was fixed up in your Museum, the other was left at the lodgings of one of your very useful, ingenious members, J. H. DE MAGELLAN, for the purpose of some experiments, and also for impregnating strongly the needles of sea-compasses. Here it was accidentally destroyed by fire, and the parts it consisted of rendered almost wholly useless. A new one has, however, been made, and impregnated with the magnetical power, by the ingenious gentleman abovementioned, according to the method of Dr. KNIGHT. It has acquired a considerable degree of magnetic force, by being placed in the polar line with the other part of this machine that was unhurt, and where in time it will, perhaps, acquire a considerable degree of magnetic energy.

I wish it had been in my power to have given a minute and pertinent detail of my deceased friend's discoveries in this branch of knowledge. He acquainted me, it is true, at different times in conversation, of the progress he had made in these discoveries; but, as I then thought he intended to leave behind him an exact account of his experiments and researches and their result, I rather listened to his relations as matters of instructive amusement, not thinking it would ever be necessary for any other person than himself to give the public an account of his labours. Indeed, there are many useful memorials of his, on this subject, in your own collections, to which I must refer the inquisitive reader. I shall only mention
some

some circumstances, relative to this machine, which I do not know have been related either by himself or any other person.

The first thing, I believe, that engaged the doctor's attention more particularly to magnetism, was the accident that befel a ship's compass from lightning; and of which, I think, the doctor gave a very circumstantial account to the society. This affair led him to consider the structure of the compass more minutely. He procured compass-cards ready-armed, as it is called, from different makers both at home and abroad. He found most of the needles strangely erring from due polarity; some being many points to the West, others as many to the East, of the right position. Amongst them all there was only one, which to him seemed constructed on a rational plan, and was of French make, procured from Marseilles; but even this was not without very evident faults.

To fix upon the proper form of a needle through which the magnetic effluvia could pass with the least interruption, to give the needle such a degree of hardness as to retain the magnetic influx the longest, and with the greatest force, were material objects; and, I imagine, a view to have such a degree of magnetic power at his command, as to force the magnetic virtue through the most consolidated bars, was his first inducement to try, whether he could not collect such a magazine of magnetism, as would be sufficient for every purpose of this kind, and at the same time exhibit some new phenomena in

physics yet undiscovered. With this view he planned and executed the machine, described at the end of this relation.

His first attempt, however, was much smaller; a few bars were laid in the due course of the magnetic flux, and impregnated by constant attrition. To these other bars were added successively, after they had been impregnated, both by the force he could give them by attrition, and what he could derive from the preceding stock collected in the bars. To these he added still fresh bars, till he had formed the whole mass as it is now presented to you, and resting on wheels and pivots, in such manner as to be easily manageable for the purpose of impregnating the needles he was employed to see prepared, for the service of government, and others, who had generosity enough to think, that the compass, on which depended the lives of the ship's crew, could not be made too perfect, and that it deserved a reasonable compensation. It is to the doctor's ingenuity and indefatigable attention to this useful instrument, that it has acquired amongst us a degree of perfection unknown to our predecessors. .

When the machine was completed, he still was adding continually to its power. He impregnated every single bar of which it is composed, by repeated attritions, and applied it to the remaining bars in their magnetic position. After this operation, he always found its efficacy, for a season, considerably diminished; for the efflu-

via

via of each bar, though increased in virtue, seemed not immediately to have acquired a communication with each other. However, it grew always more powerful after each of these operations; and it is more than probable, if a person could be found, who, with equal patience and skill, would, at proper distances, repeat the same process, that the present machine would acquire a degree of force superior to what the original ever possessed, for much depends upon time and a due position. If to these was added a fresh impregnation of each single bar, by the means hitherto made use of, you would probably possess a larger fund of magnetic power, than exists in any artificial magnet now in being.

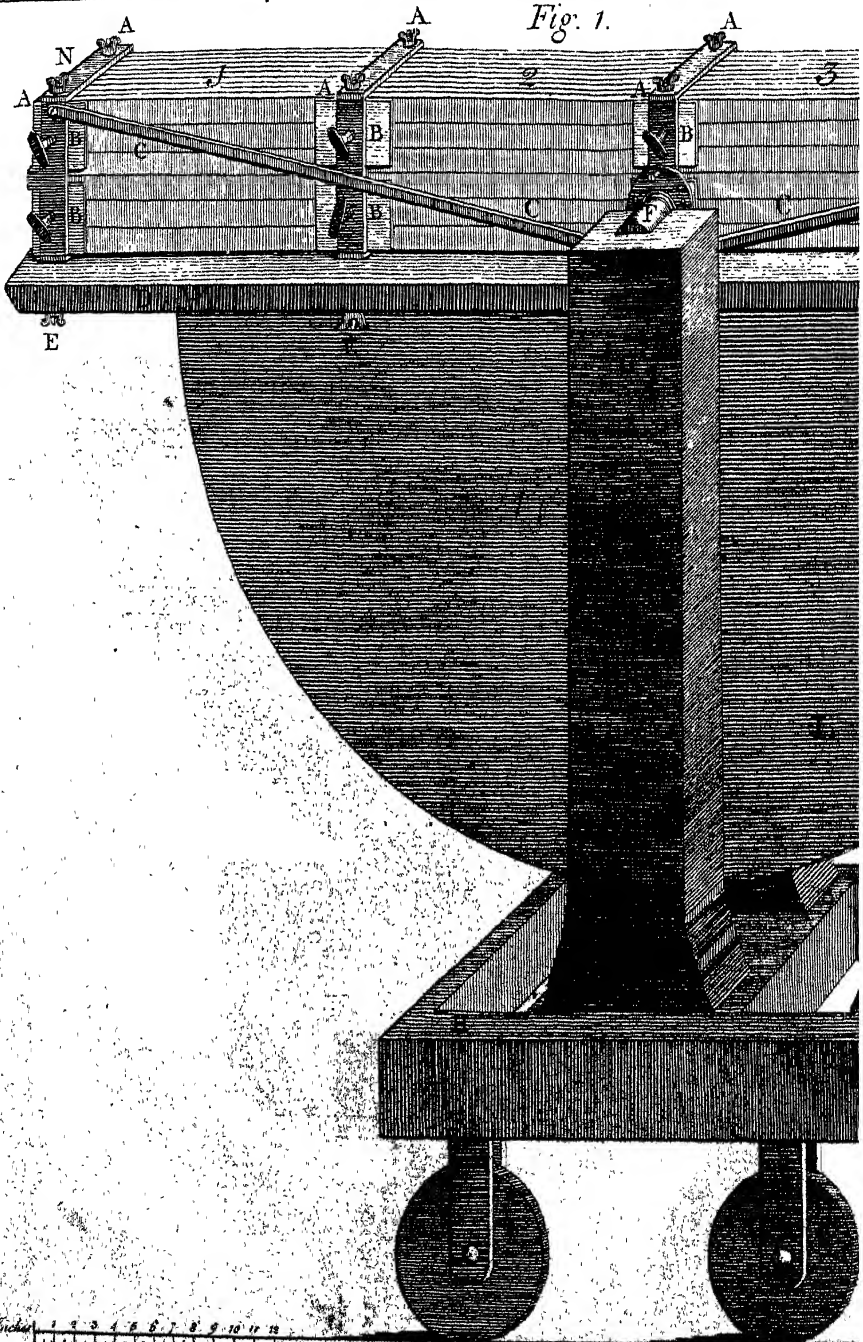
But if this cannot be obtained, if an able person cannot be prevailed upon to renew its vigour in this manner, it might possibly afford the curious some satisfaction, to know, whether, in its present state, it loses any force, or acquires fresh virtue; to know, with some degree of precision, how much weight it will now suspend; and to observe annually its variation. I need not suggest, that a trial of this nature demands no small attention. Even the motion of a carriage in the street, though at such a distance as the society's apartments, will make a considerable variation.

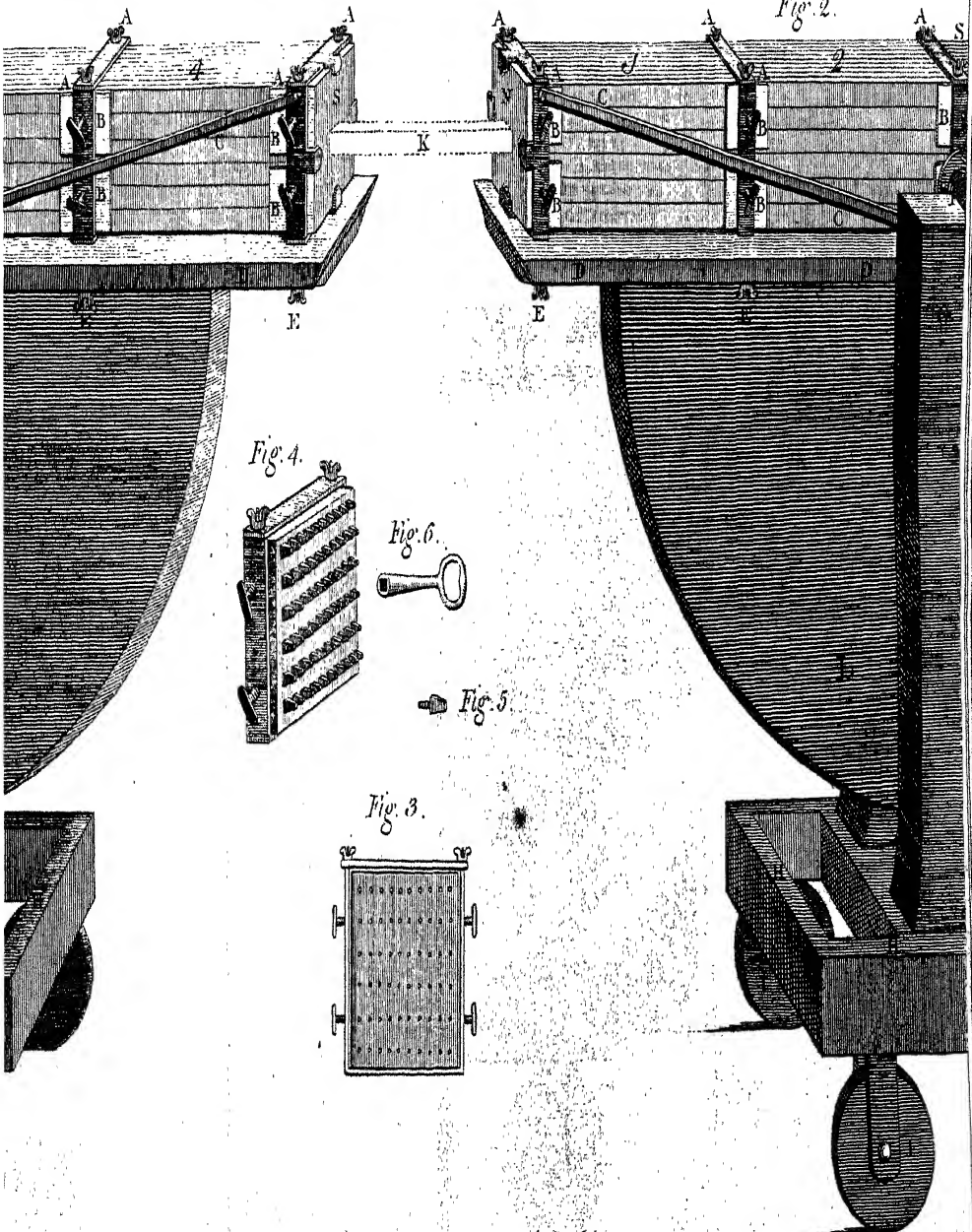
I do not know, that the doctor left behind him any description of a composition he had made to form artificial load-stones. I have seen in his possession, and many other of his friends have likewise seen, such a composition; which

retained the magnetic virtue in a manner much more fixed, than either any real load-stone or any magnetic bar, however well tempered. In the natural ones he could change the poles in an instant, so likewise in the hardest bars; but in the composition the poles were immoveable. He had several small pieces of this composition, which had strong magnetic powers. The largest was about half an inch in breadth, very little longer than broad, and near a quarter of an inch thick. It was not armed, but the ends were powerfully magnetic; nor could the poles be altered, though it was placed between two of his largest bars, and they were very strongly impregnated. The mass, was not very heavy, and had much the appearance of a piece of black lead, though not quite so shining. I believe he never divulged the composition; but, I think, he once told me, the basis of it was filings of iron, reduced by long-continued attrition with water to a perfectly impalpable state, and then incorporated with some pliant matter, to give it due consistence. Perhaps some of his acquaintance may have been more fully informed of this circumstance; and it may be rendering great aid to future enquirers, to know every thing that can be collected relative to so curious a subject.

Lest the machine itself should again be exposed to a like accident with that which destroyed a part of it, I thought an exact representation of it, and its several parts, might be the best means of preserving it to future times, if inserted in the *Transactions of the Royal Society*.

Fig. 1.





Scale of 6 Feet.

EXPLANATION OF THE PLATE.

Plate VII. shews the magazines according to the doctor's last disposition of them. The two being perfectly alike, therefore fig. 2. contains only the half of one of them. Each magazine consists of 240 bars, disposed in four lengths, marked 1. 2. 3. 4.; every length containing sixty bars, placed in six courses or layers, in contact one on another; and ten in each course, placed side by side, in contact also. The bars being very nearly of a size, the ends of those in one length are in contact with the corresponding ends of those in the adjacent lengths. The magnetical North-ends of these bars, in each magazine, are all directed one way towards N; and the South-ends the contrary way towards S; thick plates of iron cover these ends N and S; the junction of the ends of the bars fall under the brass braces AA.

As it has been found difficult, after the final hardening of these bars, to preserve among them a perfect equality in size; therefore, the contact of their sides are perfected by thin iron plates BB, slipped in between the braces AA and the junction of the ends of the bars: and these plates BB, being pressed by the screws passing through the sides of the braces AA, keep the ends of the bars in as close contact as their figures will permit; and, that the bars may be kept end to end in contact, the iron plate at the North-end in fig. 1. and at the South-end in

fig. 2. is perforated with sixty holes, one against the end of each bar, as shewn at fig. 3. with a screw fitted to each hole, as shewn at fig. 4.: every screw having a square head as at fig. 5. may, by help of the key fig. 6. be turned, and, by pressing against the end of the bar in the fourth length, force it against its abutting bar in the third length, and so on till the bars, and to end, are brought into contact and kept so. The braces are in two pieces; the sides and bottom in one; and the other piece forms the top AA, which is held close to the bars by the screws passing through it into the upright sides of the braces; and, to keep the braces at N and S steadily in their places, the two long braces CC are affixed.

As each of these magazines weighed about 500 lbs. it became necessary to have them so placed as to be conveniently used. The doctor, therefore, by screws fixed the braces, containing the bars, to a strong mahogany plank DD, about $1\frac{3}{4}$ inches thick; the screws passing through the plank entered the bottom parts of the braces AA. Against the middle of the whole length, two strong brass plates are well fixed to the sides of the plank; to these brass plates are fixed two cylindrical gudgeons F, which projecting from the sides, like the trunnions of a cannon, lye in the sockets of the standard G, whereby the magazine easily turns, as on an axis; and is so well poized as to stand in any inclination of the line NS; and in this the equilibrium is assisted by the strong mahogany semi-circular pieces LL, fixed in a vertical position to

the

N. O. 2

the middle of the under part of the plank DD, on which the magnetic apparatus rests. The standards G are fixed to the square frame HH, and the whole supported on the four trucks II, whereby the two magazines are easily brought end to end, or set at a convenient distance, for as to admit a bar K, to be placed between the ends, to be made magnetical.

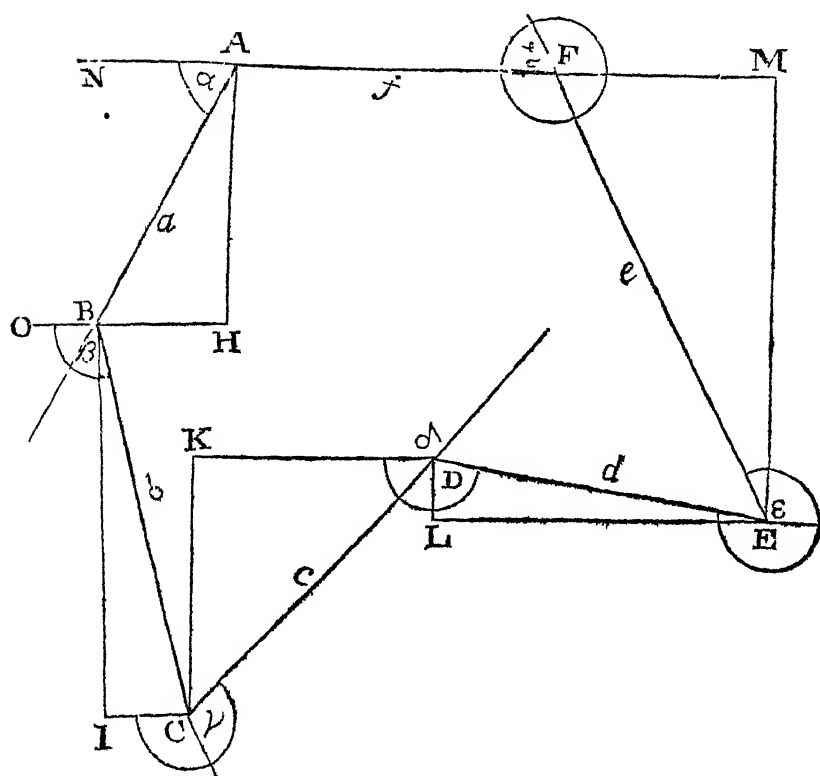
XLIII. *A Demonstration of Two Theorems mentioned in Art. XXV. of the Philosophical Transactions for the Year 1775. In a Letter from Charles Hutton, Esq. F. R. S. to the Rev. Dr. Horsley, Sec. R. S.*

TO THE REV. DR. HORSLEY, SEC. R. S.

REV. SIR,

Woolwich Warren, Royal Mil. Acad.
April 17, 1776.

R. June 27, 1776. **T**HE following is a demonstration of the two theorems mentioned in Art. XXV. of the Philosophical Transactions for the year 1776. If you think them proper to be inserted in the next volume of Transactions, be pleased to communicate them to the Royal Society for that purpose, from, &c.



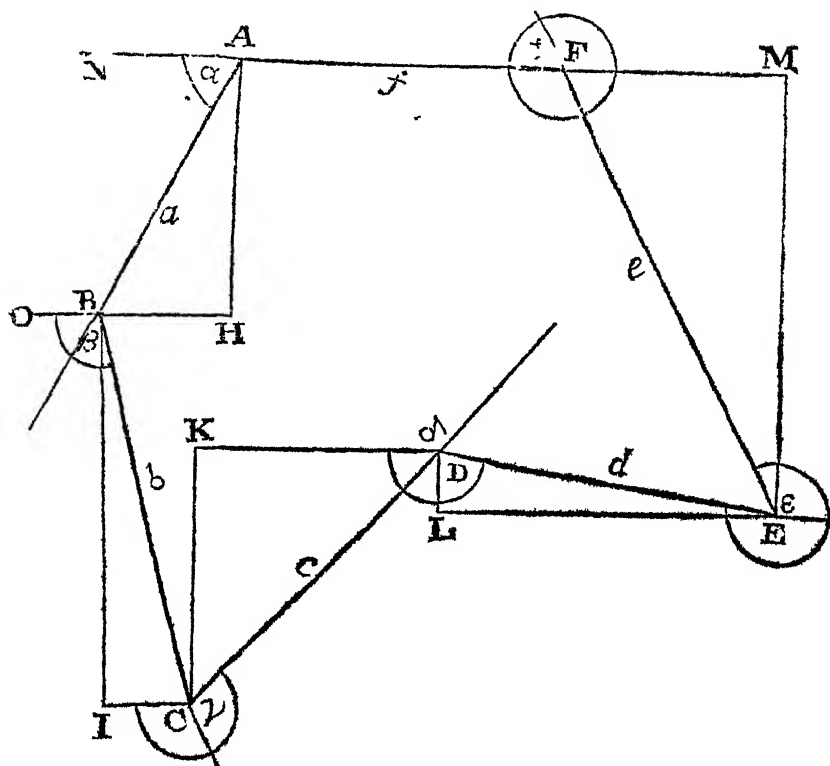
THE DEMONSTRATION.

Parallel to one side, AF, of the polygon ABCDEF, through all the angular points, draw BH, CI, DK, EL, FM. Then are the several angles, which are made by these parallels and the adjacent sides of the figure, respectively equal to the sums of all the exterior angles of the figure, from the beginning to the place of each angle; by the beginning is meant either extremity of the side AF. Thus the $\angle NAB = \alpha$, the $\angle OBC = \alpha + \beta$, the $\angle ICD = \alpha + \beta + \gamma$, the $\angle KDE = \alpha + \beta + \gamma - \delta$ (for here δ , being without the figure, is subtracted instead of being added)

added), the $\angle LEF = \alpha + \beta - \gamma - \delta + \epsilon$, &c. All this is evident from Eucl. I. 27.; the angles, &c. &c. of being those, which are measured by the little arcs described about each angular point in the figure.

I. If right lines, AH, BI, CK, DL, EM, be drawn from the angular points, perpendicular each to the parallel which passes through the next angular point, the sums of the perpendiculars, drawn in contrary directions, the one upwards, and the other downwards, will be equal. And each perpendicular will be a fourth in proportion with the radius, that side of the polygon which is adjacent both to the perpendicular and the parallel on which it falls, and the sine of the sum of the external angles taken to that inclusively from which the perpendicular is drawn. Thus, $AH + BI + DL = CK + EM$, and $\text{rad. sin. } \alpha = AB : AH$; and $\text{rad. sin. } \alpha + \beta = BC : BI$, and in like manner of the rest. Take the value, therefore, of each perpendicular by these analogies, putting unity for the radius, and add the sum of all that are drawn upwards from it to the sum of all that are drawn downwards, and the remainder, which is equal to 0, is the first equation; thus, $AH = a \times \text{f. } \alpha$ (for $\angle ABH = \angle \alpha$); $BI = b \times \text{f. } \alpha + \beta$ (for $\angle ICB = \angle HBC = \text{f. of its suppl. } OBC \text{ or } \alpha + \beta$); in like manner, $CK = -c \times \text{f. } \alpha + \beta + \gamma$; $DL = d \times \text{f. } \alpha + \beta + \gamma + \delta$; $EM = -e \times \text{f. } \alpha + \beta + \gamma - \delta + \epsilon$, &c. the last perpendicular will always be = 0, because the sine of 360° , or of $\alpha + \beta + \gamma - \delta + \epsilon$ is nothing. Hence $a \times \text{f. } \alpha + b \times \text{f. } \alpha + \beta + c \times \text{f. } \alpha + \beta + \gamma + d \times \text{f. } \alpha + \beta + \gamma + \delta + e \times \text{f. } \alpha + \beta + \gamma - \delta + \epsilon = AH + BI - CK + DL - EM = 0$, which is the first equation.

II.



II. In like manner it appears, that the intercepted parts BH, CI, DK, EL, FM, AF, of the parallels before-mentioned, are equal to the several corresponding sides drawn into the cosines of the same sums of the exterior angles (the radius being 1.); and because $BH - CI - DK - EL + FM + AF = 0$, therefore $a \times \cos. \alpha + b \times \cos. \alpha + \beta + c \times \cos. \alpha + \beta + \gamma + d \times \cos. \alpha + \beta + \gamma - \delta + e \times \cos. \alpha + \beta + \gamma - \delta + \epsilon + f \times \cos. \alpha + \beta + \gamma - \delta + \epsilon + \zeta = 0$, which is the second theorem. Or, for the last term, $f \times \cos. \alpha + \beta + \gamma - \delta + \epsilon + \zeta$, of this latter theorem, might be substituted its value f only.

EXPERIMENTS

MADE IN ORDER TO ASCERTAIN

The Nature of some Mineral Substances;

AND, IN PARTICULAR, .

To see how far the Acids of Sea-Salt and of Vitriol contribute to mineralize Metallic and other Substances.

BY PETER WOULFE, F. R. S.

Who was nominated by the President and Council to prosecute Discoveries in Natural History, pursuant to the Will of the late HENRY BAKER, Esq. F. R. S.

E X P E R I M E N T S, &c.

Gray's-Inn,
April 25, 1776.

R. June 29,
1776.

THE late HENRY BAKER, Esq. a worthy member of this honourable Society, full of laudable zeal for the advancement of natural knowledge, has most generously bequeathed the sum of one hundred pounds, for the use of this respectable body; the interest of which he intended should be annually applied for the benefit of such member whom the Council of this Society thought proper to nominate, for the task of making discoveries in natural history. These gentlemen have this year done me that honour, which I most gratefully acknowledge; and in consequence of this appointment, I have made experiments on a variety of mineral substances.

IN order to ascertain how far the method, I proposed to pursue in the following experiments, was calculated for the purpose of making a proper analysis of the minerals I here treat of; and particularly to discover the existence of the acids of salt and vitriol in them; I thought it expedient to make some artificial preparations, which I judged to be similar to the natural substances, and I submitted them to the same trials, and in similar quantities; for as many of the minerals, which are the subject of the present paper, are exceeding scarce, I was under the necessity of using in these experiments smaller quantities of them, than I should otherwise have em-

[*a*] With that intent, I dissolved half an ounce of refined silver in pure *aqua fortis*, and made a *luna cornea*, by adding to it a solution of sea-salt in water; this was well edulcorated, dried first in the air, and afterwards, with a strong heat, but not so great as to melt it; the *luna cornea*, thus obtained, weigh five drams and one scruple, which is one-third more than the original weight. I must here observe, that I have used Troy-weights in all these experiments.

[*b*] The like quantity of the same silver, dissolved in the same acid, and precipitated with a solution of tartar of vitriol, and treated as at [*a*], weighed five drams and twenty-two grains. It is worthy of observation, that if *sal polychrest* be made use of in the place of tartar of vitriol, its precipitate will not exceed three drams and fifty

four grains; but if acid of salt, or a solution of sea-salt, be added to the washings of this precipitate, the remainder of the silver is precipitated, and forms a *luna cornea*. This shews, that there is a difference between tartar of vitriol and *sal polychrest*, notwithstanding what chemists think to the contrary; and indeed, upon trial, I found the *sal polychrest* to contain a small portion of liver of sulphur. Silver dipped into a warm solution of this salt becomes instantly black; thus the *sal polychrest* may be readily distinguished from tartar of vitriol, which does not tarnish silver.

The precipitate of silver, by tartar of vitriol put on a red-hot iron, melts and grows liquid like *luna cornea*. The precipitate of silver by *sal polychrest*, treated in the same manner, does not grow so liquid, but boils up and at last dries.

[c] Half an ounce of lead, dissolved in *aqua fortis*, and precipitated by a solution of tartar of vitriol,edulcorated and dried, weighed five drams and a half.

[d] The like quantity of lead, dissolved as at [c], precipitated with a solution of sea-salt,edulcorated and dried, weighed only half an ounce and eighteen grains. A solution of tartar of vitriol, added to the washings of this precipitate, caused a fresh precipitation, which, after edulcoration and exsiccation, weighed forty-two grains. Hence we see, that lead united to acid of salt is soluble in water, and on that account so small a quantity of precipitate was obtained; but the tartar of vitriol precipitating its washings (for the acid of vitriol does the same) shews, that

that lead has a greater affinity with this acid than with that of sea-salt; it also shews, that this combination of lead with acid of vitriol is insoluble. That most excellent chemist Mr. MARGRAF, in his experiments on the Bolognian *phosphorus*, has shewn how the Bolognian stone and other such spars, as well as the *gypsa*, are decomposed by fixed alkalies; but, as it was necessary to employ an excess of alkaly to decompose them thoroughly, the quantity of neutral salt he thereby obtained could not be accurately ascertained, on account of its mixture with the alkaly. I so far availed myself of this learned chemist's method; but contrived to remove that inconveniency, in such a manner that the excess of alkaly was separated, and the neutral salt left pure. This consists in saturating the salt with distilled vinegar, evaporating the mixture slowly to dryness, and dissolving the *sal diureticus*, formed by the combination of the vinegar and excess of alkaly, in rectified spirit of wine, for this salt is very soluble in it; whereas the combinations of the acids of vitriol and of sea-salt, with either vegetable or marine alkaly, are no way soluble in this spirit. I must here observe, that no fixed alkaly is quite free from neutral salt; but the purest is that made with good tartar. Two drams of the alkaly, I made use of for my experiments, contained two grains of tartar of vitriol, for which I made an allowance in all my experiments. The quantity of neutral salt in any alkaly may be accurately ascertained by this method; and I can take upon me to say, that

that a single grain of neutral salt in an ounce of *alkaly* may be discovered by this process.

EXPERIMENT I.

Two drams of the horn silver [*a*] were well ground in a glass mortar, with an equal quantity of fixed alkaly of tartar, and distilled water enough to make the mixture of a soft consistence; it was then taken out of the mortar, and deprived of its moisture in a china cup fixed in a sand heat. The mixture was then powdered and put into a common green ounce phial, and this into a crucible, and surrounded with sand up to its neck. The crucible was fixed in a proper furnace, and a fire made round it, which was increased by degrees until the phial became of a dull red colour, in which state it was kept for an hour. The crucible was then taken out of the fire, and, when quite cold, the phial was broken; care was taken to separate the matter from the broken bits of glass. This matter was of a spongy texture, and would not powder, but flatted in the mortar, and then shewed its silvery appearance; it was, therefore, cut into thin slices and digested with four ounces of distilled water; the solution was poured off and filtered, and four ounces more of the like water added to what remained undissolved; this also, after digestion, was poured off, filtered, and added to the first solution. By this means the silver was deprived of all its saline part. The solution was evaporated slowly to about one-half, and then saturated with distilled vinegar; this was afterwards filtered, gently

evaporated to dryness, and freed from its *sal diureticus* by three lotions with rectified spirit of wine. The neutral salt here remaining was dissolved in a small portion of distilled water, then filtered and put into a wine-glass, covered with filtering paper, to keep out the dust. In about three months it was quite dry, and consisted of small, flat, cubical crystals of regenerated sea-salt, which altogether weighed fifty-five grains.

EXPERIMENT II.

Two drams of the precipitate of silver, by tartar of vitriol [*b*], treated as in the first experiment, produced one dram and seven grains of brown tartar of vitriol.

EXPERIMENT III.

Two drams of precipitate of lead, by tartar of vitriol [*c*], treated as in the first experiment, produced one dram and five grains of brown tartar of vitriol.

EXPERIMENT IV.

Two drams of precipitate of lead, by sea-salt [*d*], afforded one dram and one grain of cubical crystals of regenerated sea-salt.

EXPERIMENT V.

Two drams of *mercurius dulcis*, treated in the like manner, produced thirty-eight grains of cubical crystals of regenerated sea-salt.

EXPERIMENT VI.

Two drams of turbith mineral (made by precipitating a solution of quicksilver in acid of nitre by a solution of tartar of vitriol, welledulcorated and dried), treated as in the first experiment, produced thirty-six grains of brown tartar of vitriol.

In the first and second experiments, the silver regained its metallic form; in the third and fourth the lead was reduced to the state of massicot; and, in the fifth and sixth the quicksilver was totally dissipated.

From the foregoing experiments it is clear, that combinations of the acids of sea-salt and of vitriol with silver, lead, and quicksilver, are decomposed by these means; and also, that the quantity of neutral salt, which their acids form by combination with the alkali, is ascertained. Having premised these experiments on artificial substances, I now come to those made with natural ones, which are the subject of the present paper.

OF NATIVE HORN SILVER.

Horn silver is found of various colours, *viz.* green, yellow, brown, purple, and also black. When crystallized, it forms perfect cubes; it readily melts when put upon a red-hot iron, but does not smoke; it may be easily cut with a knife, for it is somewhat malleable; we must, however, except the black sort, which is brittle, and may be powdered. Some authors will have it, that

this mineral is composed of silver, sulphur, and arsenic; some, of silver sulphur, arsenic, and fixed alkaly; others, of silver, arsenic and acid of sea-salt; but Mess. CRONSTEAD and LE SAGE assert, that it is composed of silver and acid of salt only. I know of no experiments that have been made public with a view to determine this affair, except those of M. LE SAGE; but he and I differ widely in the result of our experiments.

EXPERIMENT VII.

Two drams of brown, native horn silver, cut into very thin slices, and well ground with the like quantity of fixed alkaly of tartar and a little water, treated as in the first experiment, produced forty-three grains of neutral salt, composed of flat cubic crystals of regenerated sea-salt, intermixed with brown crystals of tartar of vitriol; this last did not appear to be above one-third of the first. The brown colour of the horn silver is owing to an ochre of iron; a thin slice of it, viewed through a magnifying glass, looks in some parts of a smooth pearly colour, but in others of a powdery brown one.

The silver, which remained after this operation, had the same texture and silvery appearance as that of the first experiment. It was digested with distilled vinegar, then filtered and evaporated to one-fourth, with a design to dissipate the excess of acids. A fresh infusion of galls mixed with it, becomes of a dark purple colour, which precipitates; an indomestible proof of the iron it contains;

Silver, in its metallic state, is not soluble in distilled vinegar.

EXPERIMENT VIII.

Two drams of the pearl-coloured horn silver, treated with the like quantity of fixed alkaly as in the first experiment, produced fifty-one grains of neutral salt, composed of flat cubic crystals of regenerated sea-salt, with a mixture of brown crystals of tartar of vitriol; this last, in appearance, is about a fourth or more of the first. The silver here remaining had the same appearance as that of the first experiment; it also gave marks of its containing iron, but not near so great a quantity as that of the seventh experiment.

EXPERIMENT IX.

The brittleness and colour of the black horn silver made me at first doubt of its being horn silver; and I thought, that, if it contained any, it must be mixed with some other mineral substance. Volatile alkalies having the property of dissolving all combinations of silver with acids, I availed myself of that well-known property on this occasion. I took, therefore, seven drams of the black horn silver, and digested it at three different times with a large portion of volatile spirit of hart's-horn; this spirit I preferred to that of *sal ammoniac*, as I knew it to be free from acid of salt. The three solutions were mixed, filtered, and slowly evaporated to dryness, and produced two drams and two scruples of a dark slate-coloured horn silver.

silver. Forty-six grains of artificial horn silver [*a*], dissolved in spirit of hart's-horn, and dried in the same slow manner, increased only two grains in weight, and was of the same dark colour; owing, no doubt, to a small portion of volatile spirit adhering to it.

The two drams and two scruples of horn silver, obtained by means of the spirit of hart's-horn, were well mixed with an equal quantity of salt of tartar; and being treated as in the first experiment, produced one dram and eleven grains of neutral salt, consisting of flat cubical crystals of regenerated sea-salt, intermixed with brown crystals of tartar of vitriol, which last appeared to be in less proportion than in the former experiments. The silver, after this operation, had the same spongy, silvery appearance as in the former experiments. The undissolved part of the horn silver, which remained after its horn silver was extracted by the spirit of hart's-horn, retained its black colour. It was calcined in a crucible, and, during the calcination, a sulphureous smell was observed; the calcination was continued until this smell ceased. It had now an ash-colour, silvery appearance, and, during the operation, slightly caked together, which made me think it had yet some horn silver; I therefore digested it with more spirit of harts-horn; and, having evaporated the solution slowly to dryness, I obtained thirty-four grains of horn silver; the undissolved part here remaining, being melted with black flux, produced two drams and a half of pure silver, so that black horn silver is composed of horn silver and sulphurated silver. The solution of
the

the Nature of some Mineral Subj.

the three sorts of horn silver in spirit .

were as colourless as water; a proof they contain no copper.

To give a proof of the existence of the acid of salt in the horn silver, I took four grains of the cubical crystals obtained in the seventh, eighth, and ninth experiments, and having put them into separate wine-glasses, poured a little oil of vitriol on each, which made them all boil up, effervesce, and send forth copious fumes of acid of salt, just as the like quantity of sea-salt would have done.

In order to give also a convincing proof, that the horn silver contained acid of vitriol, I availed myself of M. MARGRAF's discovery, who says, that a solution of calcareous earth in acid of nitre is precipitated by a solution of tartar of vitriol; for the acid of vitriol forsakes its alkaly to unite, and form a selenite with the calcareous earth. I therefore dissolved twelve grains of tartar of vitriol in distilled water, and having filtered the solution, I added to it a sufficient quantity of a solution of chalk in acid of nitre, which caused a precipitation; this precipitate, beingedulcorated and dried, weighed seven grains: this served as a comparative experiment. It is probable, from the small quantity of this precipitate, that the whole of the tartar of vitriol was not decomposed. It shews, however, that seven grains of this precipitate require twelve grains of tartar of vitriol to their formation.

The neutral salt of the seventh experiment, treated in the like manner, produced eight grains of selenite; that
of

of the eighth, seven grains and a half; and that of the ninth ten grains.

In calcining the different specimens of horn silver with the alkalies in the foregoing experiments, I could perceive no smoke or smell issue out of the phials. Hence I concluded that the horn silver contained no arsenic; and that none of it was dissipated during the operation.

From the foregoing experiments it appears, that the horn silver is composed of silver united to the acids of salt and of vitriol; and that this last is nearly one-third of these first.

O F H O R N M E R C U R Y.

I discovered this mineral about four years ago, when I was collecting minerals at Obermoschel, in the Dutchy of Deux Ponts. I have since seen it in a fine collection abroad; but no one suspected what it was until I had made it known, for it was taken for an insignificant spar. I have found this mineral of three colours; white, and of a shining brightness; yellow, and also black; this last owes its colour to a mixture of minute particles of live quicksilver. This substance crystallizes in various forms; but the crystallization is too small to be described without the help of a microscope.

E X P E R I M E N T X.

I took three drams of horn mercury, picked as clean as possible from the cinnabar and stony matter to which it adhered, and treated it with two drams of salt of tartar

as in the first experiment. The quantity of neutral salt here obtained was only half a dram, and it was composed of flat, cubical crystals of regenerated sea-salt, intermixed with about an equal quantity of brown crystals of tartar of vitriol. The quicksilver was all dissipated in the operation, as in the fifth experiment. Four grains of the cubic crystals, mixed with oil of vitriol, boiled up, and sent out copious fumes of acid of salt. The remainder of the neutral salt was dissolved in distilled water, and mixed with a sufficient quantity of a solution of chalk in acid of nitre; the precipitate, here obtained, beingedulcorated and dried, weighed eight grains and a-half. Hence we may conclude, that this mineral is composed of quicksilver united to a greater proportion of acid of vitriol, than of acid of salt.

The horn-mercury used for this experiment was intermixed with minute *globules* of quicksilver, which could not well be separated, to which the small produce of half a dram of neutral salt was owing; and, indeed, to obtain the three drams of horn-mercury employed for this experiment, it was necessary to destroy several beautiful specimens.

The ingenious M. LE SAGE, of the French Academy of Sciences, has published many experiments to shew, that the acid of salt contributes to mineralize a great variety of mineral substances; but I have tried a great number of them without obtaining an atom, either of acid of salt, or acid of vitriol. Among these are the

following, which were submitted to the same trials as in the first experiment.

White sparthose iron ore from Bayreuth.

Cornish and Bohemian tin grains.

Mendip calamine.

Semi-pellucid calamine, from Wales.

Sooty kobalt, from Saxony.

Somerfetshire mangancse; this is the only sort I ever saw which is mixed with calcareous spar, and, on that account, effervesces with acids without help of heat.

Cerussa nativa, in lump, from Lorraine; I found this to consist of a *calx* of lead, mixed with an argillaceous earth.

White, transparent, lump lead ore, from Somerfetshire.

Cat's-tooth white lead ore, from Ireland.

White lead ore, from Poulasent in Low Brittany.

Black, whitish lead ore, from Tschoppan in Saxony.

Green lead ore, from ditto.

Green lead ore, from Freibourg in Brisgau.

In all these experiments the lead was converted to massicot; but, in the two last, the massicot had a pale greenish cast, owing to iron, and not to copper: for if these green lead ores, after having been calcined with fixed alkaly and edulcorated, be digested with acid of vitriol, we obtain a greenish solution; this, being then deprived of its excess of acid by an alkaly, filtered and mixed with infusion of galls, gives a black, inky colour.

Having now tried in my manner, without success, a great number of the minerals which M. LE SAGE affirms to contain the acid of salt, it became also necessary to try his method; and I chose such substances as he says are the most replete with that article.

EXPERIMENT XI.

White, spathose, iron ore from Bayreuth, tried in M. LE SAGE's manner.

I put into a small glass retort three ounces of this mineral powder, and poured on it an equal quantity of oil of vitriol. The retort was placed in a proper reverberatory furnace, and a quilled receiver luted to it; an ounce measure of oil of tartar, *per deliquium*, was previously put into the receiver, and shaken so as to moisten all its internal parts: a very slow fire was made under the retort, such as caused no moisture to rise; and this gentle degree of heat was continued for five hours. In a little time the upper part of the receiver was lined with long spiculine crystals; and, after the operation, the oil of tartar, which was at the bottom of the receiver, was, for the most part, crystallized.

EXPERIMENT XII.

Somersetshire manganese, treated in the like manner, produced the same spiculine crystals in the upper part of the receiver; and the oil of tartar was, in like manner, crystallized. It was here necessary, on account of the

effervescence of the mixture, to make use of a larger retort, and to let it stand for twelve hours before any fire was made under it, in which time the spiculine crystals appeared.

EXPERIMENT XIII.

Somersetshire white-lead ore, treated in like manner, shewed the like appearances; but here there was only a very flight effervescence.

EXPERIMENT XIV.

Cornish tin-grains, treated in this manner, caused no change in the oil of tartar; nor was there the least appearance of crystallization in the receiver.

EXPERIMENT XV.

White spathose iron ore, distilled *per se* with a strong fire, shewed the same appearance of crystallizations in the oil of tartar and in the upper part of the receiver, as when distilled with oil of vitriol; see the eleventh experiment.

I now saturated each of the crystallized alkalies of the eleventh, twelfth, thirteenth, and fifteenth experiments, with distilled vinegar, and having filtered them, they were slowly evaporated to dryness, and dissolved with rectified spirit of wine, as in the first experiment, but no neutral salt was left; from hence we may certainly conclude, that these minerals contained neither acid of salt, nor acid of vitriol. The cause of the crystallization of

the oil of tartar is owing to the phlogistic fixed air of the the minerals; but of this I shall treat more fully in another paper; and will now only add, that chalk, distilled *per se*, with a strong fire, makes the oil of tartar in the receiver also crystallize. I must, however, own, that no crystallization is observed in the upper part of the receiver; nor was it to be expected, on considering the moisture which the chalk affords in distillation.

M. LE SAGE says, that the crystallizations in the upper part of the receivers in his experiments were composed of cubic crystals; but in all mine they were spiculine.

By the foregoing experiments it appears, that silver and quicksilver are the only substances which are mineralized by the acid of salt, and that they are also combined with acid of vitriol.

Though the result of my experiments has been very different from that of the same substances examined by M. LE SAGE, yet I have too high an opinion of him, and know him too well, to call his veracity in question. I am rather inclined to suspect that this difference may proceed, from his having used oil of vitriol that contained acid of salt. Mr. HOLKER, who prepares this acid in France, owned to me, that he had, in his first trials, mixed sea salt in the preparing of it, with a view to increase its quantity. Might not M. LE SAGE have made his experiments with such an acid; and of consequence obtained acid of salt from the substances he tried.

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will

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E R R A T A.

Page	Line
15.	1. <i>n.</i> <i>for</i> branche <i>read</i> branches.
120.	12. <i>for</i> part this <i>read</i> part of this.
231.	14. <i>for</i> expement <i>read</i> experiment.
32.	14. <i>for</i> suphurcous <i>read</i> sulphureous.
242.	3. from the bottom, <i>for</i> oommon <i>read</i> common. 2. from the bottom, <i>for</i> cne <i>read</i> one.
262.	last word of the page, <i>for</i> Both <i>read</i> These.
416.	last word of the page, <i>for</i> Nor <i>read</i> Now.
445.	12. <i>for</i> unhealthy <i>read</i> unhealthy.
498.	in the note, <i>for</i> they do not admit a dark hair <i>read</i> they do not by choice admit a dark hair.



Philipp Carl Hanthöpe.